NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 6. Vol. I. AUGUST, 1892.

NOTES AND COMMENTS.

The Imperial Institute.

THE recent announcement of the issue of a detailed prospectus of the aims and methods of the Imperial Institute, was welcomed by many who hoped that this would at last lift the veil from the great South Kensington "Hall of Mysteries." We fear, however, that most of those who had hoped to gain some clear ideas upon this subject are doomed to disappointment. The prospectus is a pamphlet of 64 pages, but by far the greater part of this is devoted to lists of the illustrious ones who serve as governors and on committees, to preliminary correspondence which we read years ago, to glowing descriptions of the scenes of "spectacular magnificence" at the "gorgeous pageant" when the foundation stone was laid, and to elaborate details regarding the architecture of the building. Only a modest 7 pages are devoted to the section on the "objects and purposes," and the statements here are written in a style that combines the foggy precision of a limited liability company memorandum with the profusion of promises of an election address. Among other benevolent functions, the Institute intends to undertake "the collection and dissemination of such information relating to trades and industries, to emigration, and to the other purposes of the charter as may be of use to the subjects of the Empire." The point in the programme on which stress is first laid is that of the formation of a collection of the raw products; by this, in association with libraries and conferences, it is hoped that the Institute will become "a central source of information upon all matters relating to the natural and industrial resources, the trades and handicrafts, and the commerce of every part of the Empire." Branch museums and

sample rooms are to be widely scattered; systematic colonisation is to be furthered; commercial education encouraged, and industrial exhibitions and conferences arranged. As if this were not enough, the Institute promises to do "anything incidental or conducive to carrying into effect all or any of the foregoing purposes," and all this on an endowment of £4,000 a year. The only item in the programme that does appear practical is that which proposes to use the Institute as a South Kensington club, with a "special refreshment room for Fellows," combined with all ordinary club privileges.

To all this there is simply one fundamental objection. It is the right thing in the wrong place. The original scheme was well worthy of the occasion it was intended to commemorate. Our colonial empire has been founded by commerce and for commerce, and upon this, more than on race or language, it depends for its continued union. There could, therefore, have been no happier method of commemorating the era during which our colonies have made their greatest progress, than by the establishment of an Institute whose duty it should be to prevent industrial effort running to waste, and to substitute for the hostility of commercial competition the belief that "the good of each is the common good of all." For such an Institute, however, properly to fulfil the purposes for which it was designed, it was absolutely essential that it should be placed right at the commercial centre of the empire; that, of course, fixes its proper place as in the City, and, if possible, within the few acres wherein are crowded the Bank, the Clearing House, the Royal Exchange, the Stock Exchange, the Mansion House, and the Baltic. South Kensington, however, may possibly be made the centre of English culture, science, and art, but the commercial centre it can never be until Macaulay's Maoris there hold auction with their long-lost and lamented green-jade gods.

The scheme, as at first proposed, was received with enthusiasm by men who recognised its full business value, and who were prepared to back it with the wealth and organising genius of the City; and this would assuredly have been a more stable foundation than the fickle enthusiasm of popular sentiment. The moment the site was changed to South Kensington, however, the whole basis was altered. The City men at once realised that a museum of commercial samples, located seven minutes' walk from a station nineteen minutes by train from the nearest point of the City, would be about as useful to them as a ready reckoner kept at Brighton. A heavy percentage was therefore deducted from their subscriptions to found a smaller but more accessible institution, and this not only reduced the income, but introduced the disorganisation inevitable to divided interests.

The selection of South Kensington was no doubt made from the impossibility of securing a sufficiently imposing site further east; but we think, under the circumstances, it would have been better to

have erected a compact and commodious hall, such as those with which the not so very impecunious City Companies content themselves, rather than a spread out and showy palace. As it is, the building has taken nearly all the funds at the disposal of the Institute, for though an endowment yielding £4,000 a year has been retained, a further £80,000 is required for the building fund. As the chances of collecting this within a short time do not seem very hopeful, the Institute only adds another to the Kensington group of unfinished edifices. The financial condition of the Institute is not the least unsatisfactory feature in its outlook; the endowment of £4,000 a year is wholly inadequate, and the annual subscriptions of the Fellows and Colonies are not likely to give it a firm basis for its multifarious projects. The maintenance of the museum alone ought to absorb more than its income. In fact, unless Parliament be willing to vote a grant in aid, the Institute seems likely to be of most use as a Kensington club, while, as the home of fashionable soirées, it may remind a larger audience of the fate that befalls even Imperial institutions when they choose to ignore the limitations of time and place.

Right of Way across the Norfolk Broads.

The recent decision in Chancery as to the rights of fishing and shooting on Hickling Broad was awaited with considerable anxiety by many who feared that the interests of the public were being seriously jeopardised by the reckless zeal of its champions. Mr. Justice Romer's decision, however, is more satisfactory than it might have been, for not only is the right of way across the Broad to Hickling Staithe fully maintained, but there is full liberty to leave the channel. The shooting and fishing on the Broad, however, are decided to be private, which, after all, is a mere detail now that full access to it is admitted. The defendants' claim that the Broad is tidal is simply preposterous; anyone might as well maintain that the reservoirs of the Battersea waterworks are tidal and accordingly claim the right to unlimited picnic on them. If the Court had upheld the view that Hickling Broad is tidal, that term would simply have had to be redefined. It is to be hoped that visitors and riparian owners will agree better now that their respective rights have once again been clearly defined. So long as the public have undisputed right of way across most of the Broads and as much free shooting and fishing as there is in the district, it is to be hoped that they will cease to infringe the rights of the riparian owners. So far the latter have, on the whole, acted with great consideration, and if the private Broads are ever closed the fault will rest with those members of the public who have abused privileges which otherwise would not have been questioned. The Broad landlords have never exhibited the spirit shown by some of the suburban landlords, to one type of which attention is this month called in our correspondence columns.

Newspaper Science.

In our first number we had occasion to protest against the feebleness of most of the stuff that usually passes in the daily journals for "Science." We are therefore glad to be able to congratulate the *Daily Chronicle* on not only maintaining a weekly column of "Science Notes," but on not leaving the office boy to write them. In the notes, however, of June 30, there is a paragraph on the origin of the Great Lakes of North America, in which Dr. Spencer's observations on the directions of the ice scratches are said to put the glacial theory out of the field. But this is hardly fair to Professor Newberry who knew all these facts, and whose hypothesis was based to a large extent upon them. He maintained that the lake basins were old river channels broadened out by the ice crossing them and constantly wearing back the southern bank against which it was piled up.

The Laws of Hybridism.

Professor Romanes explained, at a recent meeting of the Zoological Society, that speculations on heredity were in inverse ratio to the experiments. A comparison of the following extracts from the last number of Natural Science shows that, however few those experiments may be, the knowledge of them appears to be in the inverse ratio squared :—

Professor Romanes.

Natural Science, *July*, p. 398.

The experiments made "prove the error of those writers who assume that an act of fertilisation consists in the male and female elements intimately blending together, after the manner of a mere mechanical mixture, so that the offspring always presents characters more or less intermediate between those of its parents."

Professor Giard.

Natural Science, *July*, p. 358.

"*As all biologists know*, the progeny obtained by the union of two races cannot be intermediate between the two progenitors, but resemble closely one or the other. For instance, in a union between a female white mouse and a wild gray male, one constantly obtains young, and some of whom are completely gray others completely white."

Texan Geology.

In a paper on "The Cretaceous Covering of the Texas Palæozoic" (*American Geologist*, March, 1892), Mr. Ralph S. Tarr discusses the question whether the central Palæozoic and pre-Palæozoic region of Texas was completely covered by Cretaceous Rocks. His studies lead him to maintain that this was the case, and his conclusions thus agree with views expressed by Professor R. T. Hill.

From early Carboniferous times until the Cretaceous period, the ancient Palæozoic core of Texas has been subject to denudation. This is shown by the character of the Carboniferous conglomerates and

the subsequent uncovering by erosion of ancient bays and promontories that marked the margins of Carboniferous seas. Later on, extensive Permian rocks were laid down in inland seas.

The features marked out in the Carboniferous area prior to the Cretaceous covering are nowhere very striking; the hills recently uncovered are broad, flat-topped, and gently undulating. Thus, when the Cretaceous period began in Texas, it found a low-lying land of Carboniferous and Permian strata, undulating and nearly at its base-level, and an older Palæozoic highland district considerably diversified, but not distinctly mountainous. Upon this area the encroaching sea commenced the deposition of sands and conglomerates; then, with more complete submergence, followed higher beds of deeper water origin. This basal Cretaceous deposit is not an ordinary beach conglomerate, for it contains many limestone pebbles, and the matrix, fully one-half its bulk, is limy matter. The beds appear to have been formed from the hurriedly worked over soils and land *débris*, in a rapidly encroaching sea. Over the Permian strata they are red, agreeing with the colour of the underlying rocks.

The old Palæozoic land at present seldom reaches an elevation above 2,000 feet, and at 1,900 feet traces of the earlier Cretaceous beds have been found. The higher Cretaceous beds indicate deep-water conditions; they include thin beds that have been traced over hundreds of square miles, and a great thickness of true chalk. Their aggregate thickness is several thousand feet. These facts, taken in conjunction with the absence of littoral characters in the higher Cretaceous beds, and with the present dip of the strata in relation to the older rocks, indicate that all the higher ground must have been covered. The central highland region of Texas is at present much more elevated than the uppermost Cretaceous beds to the south-east; it was probably the first area to be upraised, and to suffer denudation, so that the streams now have adjusted themselves to the channels of the ancient watercourses.

With regard to "The Texan Permian and its Mesozoic Types of Fossils," we may state that a short memoir with the title just given has been published by Mr. Charles A. White (*Bulletin U.S. Geological Survey*, No. 77, 1891). Thirty-two species of Invertebrata are recorded, and some of these are figured in four plates. The species show a commingling of Carboniferous and Mesozoic forms.

"A Severe Critic."

In a pamphlet, printed for the author, and entitled "The Geological Map of the United States, and the United States Geological Survey" (Cambridge, U.S.A., 1892), Mr. Jules Marcou has severely criticised the conduct of the American Geological Survey. He thinks that "Ten or twelve years at most is a sufficient lapse of time to execute all the researches in the field necessary to fill up the Geo-

logical map with exact data. Then three other years to prepare the map, sections, reports, and descriptions of fossils; and in fifteen or sixteen years, at most, we should have a good geological map of the United States," &c. He estimates that the work could be done by thirty-three officers, and at a cost of about £800,000.

If only a sketch-map were wanted, the task could possibly be achieved in the manner suggested by M. Marcou, for he contemplates "a map in ten or twelve large sheets for the whole United States."

When, however, we read that "The first want and principal object is to construct a geological map . . . to be used by Government in all general questions which may arise for Industry, Commerce, Agriculture, Forestry, Mining, Railroad," &c., and that the map "should be correct in regard to chronology, determination of rocks represented in each geological basin, and also in regard to exact limits of formations," we fear that the critic has had little occasion to use geological maps for practical purposes. Even the one-inch scale of the British Geological Survey is too small for many practical purposes, and M. Marcou would have one about one-thirtieth of that scale! In fact, he is bound to admit that "it is impossible on a general map to mark distinctly the superficial deposits," and although admitting "the importance of the Mississippi river alluvial, and of the coast formations," he would leave surface geology for the most part to volunteers and free geologists, not in the pay of the United States Government. One would have thought that in matters of Agriculture, Forestry, Railroad, &c., as mentioned above, not to forget questions of Water-supply, the mapping of the Drift formations would have been as important as that of any other formations.

Again, M. Marcou complains of the "Extravagance in the Management of Publications." In this country the United States has usually been regarded as furnishing a noble example—for surely parsimony is detrimental to the growth of Science.

British Cretaceous Foraminifera.

For many years it was a matter of reproach to English palæontologists, that, although numerous monographs on Continental Cretaceous Foraminifera had appeared, no papers of any importance had been published on the Rhizopoda of our English Chalk or Gault. That reproach has now been partly swept away by the detailed report on the "Foraminifera of the Red Chalk," by Messrs. Burrows, Sherborn, and Bailey, which appeared in 1890, and by the commencement of a monograph on the "Foraminifera of the Gault," by Mr. Frederick Chapman. Both these papers have been published by the Royal Microscopical Society in their Journal, and, as they have been fully illustrated, will form most valuable reference papers for a long time to come. Of the "Foraminifera of the Gault,"

Part I. deals with the *Miliolininæ* and the *Peneroplidinæ*, and was issued in August, 1891; the second part, describing the Arenaceous forms, was published in June; further continuations are promised. Mr. Chapman has treated the subject in a most exhaustive manner; he has confined his attention mainly to the Folkestone sections, and has worked out large samples of the clay from every zone in the Gault at that place. In his papers, therefore, we get not merely a list of the species found in the Gault, compiled from lumps of clay collected at random, but a definite and exact report on the forms found at every horizon, with their distribution above or below. Thus, in dealing with *Miliolina venusta* (Karr.), we read: "This Foraminifera makes its appearance in the Gault in zone x., where it is very common; also in zone xi., 55 ft. from the top, very common; 50 ft., very common; 45 ft., frequent; 40 ft., frequent; 35 ft., frequent; 30 ft., frequent; 25 ft., very common; 20 ft., common; 12 ft., common; 6 ft., very rare." It is interesting to note the comparative rarity of some genera; thus, out of the many thousand forms examined, Mr. Chapman has only found one specimen of *Biloculina*, and one of *Spiroloculina*, and only one of the deep water form *Hormosina*. Others occur in only one of the eleven recognised zones, *e.g.*, *Haplostiche*, in zone xi., at one horizon only (20 ft. from the top). It is likely, therefore, when we possess the whole of Mr. Chapman's paper, we may be able to form a much better opinion of the formation of the Gault than we now can, and all students of the Foraminifera will await the completion of this careful monograph with interest. The Foraminifera of the Lias are also receiving attention at the hands of Messrs. W. D. Crick and C. Davies Sherborn, in the Journal of the Northamptonshire Natural History Society.

The Identification of Trees in Winter.

The *Botanical Gazette* for June contains an article by Aug. F. Foerste on the identification of trees in winter, when the flowers and leaves, on which a diagnosis usually depends, are absent. It is important to use those parts "which are certain to be present both in the young growth and in the fully matured plant, and which, during the various stages of development, . . . show essentially the same characteristics." The only part which approximately fulfils these conditions "is the crop of little twigs added each year to the tips or the sides of the branches, with the leaf-scars from the last season's leaves, and the subtended winter buds." If the length of the twig, which varies considerably, be left out of consideration, and only the larger leaf-scars and scaly buds be made use of, the result is a very striking constancy in the colour and markings of the bark, the character of the pith, the form and structure of the leaf-scars, the figures presented by the foliar bundles, and the form and structure of the scaly buds. This constancy of characteristics in individual

species is accompanied by sufficient variation among the species of a genus to allow of the determination not only of the genus but also, in the great majority of cases, of the species.

The author proceeds to discuss, in the order of their importance, the various characteristics presented by ligneous plants.

I. The determination of the phyllotaxy, as indicated by the leaf-scars. "It so happens that a classification of shrubs and trees into those with alternate, spiral, and opposite or whorled leaves gives three fairly equal sets, so that this determination at once excludes a large number." In the case of rarer phyllotaxies, *e.g.*, one-third (*Spiræa opulifolia*) or two-thirds (*Catalpa speciosa*), the identification becomes still easier.

II. The form of the leaf-scar and the arrangement of the vascular bundles seen as dots on the exposed surface. The arrangement may be circular (*Morus rubra*, *Rhus aromatica*, &c.), semicircular (species of *Fraxinus*), or horseshoe-shaped (*Ptelea trifoliata*, and also species of *Fraxinus*), varying usually with the shape of the scar. Often, however, the bundles form distinct sets, as in *Ailanthus glandulosa*, or species of *Juglans*, with a more or less heart-shaped petiole, where we find lunate sets. In other heart-shaped scars, and in most lunate ones, the bundles form small circular areas, and the number of areas in the fully-developed scar is fairly constant. Thus *Ulmus fulva*, *Spiræa opulifolia*, *Hamamelis virginica*, and others have three sets, while *Rhus toxicodendron*, and species of *Æsculus*, &c., have five.

III. The character of the winter buds. These may be concealed in the substance of the scar, or so covered by its anterior end that their development towards spring causes more or less splitting of the scar, as in *Robinia pseudacacia*. Sometimes they are sunk in the bark of the twigs, as in *Ptelea trifoliata*. Prominently-developed scaly buds can be classified into those showing exteriorly only one or two scales, or a glimpse of a third or fourth (*Liriodendron tulipifera*, *Rhus glabra*, *Tilia americana*, &c.), and those with four or more scales exposed. The latter class can be further subdivided into those with terminal buds much larger than the lateral (species of *Fraxinus*, *Juglans*, &c.), and those where such a difference is not particularly marked.

IV. The mode of termination of the branches. In the great majority of cases terminal scaly buds are always present, but sometimes the green tips of the new twigs are cast off each spring, and the place of the terminal bud is accordingly marked by a scar in the winter twigs, as in *Tilia americana* and *Ailanthus glandulosa*. In other species the tip of the branch shrivels up early, but is not cast off, the shrivelled tip remaining through the winter (*Diospyros virginiana*, and species of lilac).

V. The presence or absence of stipules, as indicated by their scars. As the stipules, when present, usually fall early in spring, a little practice is required to easily recognise their indistinct scars, which may more or less encircle the stem, as in the Tulip-tree, or as

is usual be considerably separated. Sometimes, as in *Robinia pseudacacia*, the stipules are represented by thorns.

VI. The presence of thorns in general. These often represent small axillary branches, usually supplemented by normal leaf-buds at their base, when the relative position and character of the thorns and buds may be of value for distinguishing species.

VII. The presence or absence of ridges on the bark. These are usually related to the leaf-scars, being frequently decurrent from them. The more or less rounded angles are also worth observing, as in the frequently eight- to ten-angled stems of *Sambucus canadensis*.

Besides these more important characteristics furnished by the annual growth of twigs, which "may be used in forming a sort of artificial classification," there are others which are very useful in distinguishing individual species.

The colour of the bark of twigs being usually grey or brownish, the constant presence of twigs with green bark, as in *Sassafras officinale*, or shades of red or purple, becomes a valuable feature. The character of the pith is at times useful.

Other characteristics are very good, if present. For instance, the form and character of next year's flowering buds, whether present as naked catkins, or enclosed in more or less scaly buds. The remains of last year's inflorescence may also help. The form of the bark may be of service; smooth, as in the beech, or peeling in sheets, as in the birch, or, especially in older trees, showing characteristic cracks. Finally, the habit of the shrub or tree is often very characteristic, although the general aspect, resulting from the mode of branching, may be quite different in a young and an old tree, as in the elm.

Although it must not be expected that these winter twigs, with their scars and buds, will furnish better means of distinction than the ordinary botanical ones, it is astonishing how successful they may prove. The author suggests that a ready means of distinguishing the ligneous plants would be a welcome addition to the curriculum of subjects dealt with in the botanical institutions existing in many States for the diffusion of knowledge of practical utility.

Botanical Nomenclature.

As the subject of nomenclature is now disturbing the minds of botanists, the following letter, reprinted from the *Botanical Gazette* of June, 1892, may be of interest. The last words of the well-known American systematist will seem the more opportune, as it is in the neo-American School of Botany that the disturbing influence has largely arisen. The letter was dictated by Sereno Watson in his last illness, and is as follows:

"For some time I have had a desire to give expression to my views upon botanical nomenclature. Under the circumstances I must speak briefly and somewhat dogmatically. In my opinion

botany is the science of plants and not the science of names. Nomenclature is only one of those tools which is necessary to botany, and this being the case, points of nomenclature should be subordinated to science.

"A principle of botanical convenience has been established by those who prefer one name to another on account of expediency or convenience. This principle should have a great deal of influence. It has been so recognised by the greatest botanists, and from their authority receives great weight. I prefer the word *expediency* as a better term than convenience to designate the principle, that the demands of science over-ride any merely technical claims of priority, &c.

"Priority of specific names appears to be based entirely upon one section of the code of 1867. That simply says that when a species is transferred from one genus to another, the specific name is maintained. This principle is usually understood and applied in the way that the oldest specific name has a right in all cases to be retained. It cannot fairly be so interpreted and applied, since it governs only to the extent that this should be the law, but it is not to be made an *ex post facto* law. Thus, when a transfer has been made, that ends the matter so far as the choice of a specific name is concerned, and no one is authorised to take up a different name. This practice of retaining the oldest name *under the genus*, no matter what older specific names there may be, was adopted by Dr. Gray in his later years, and by the Kew botanists, for the reason that, once established and pretty generally recognised, it would avoid the great mass of synonymy which is being heaped like an incubus upon the science. I must express surprise that Dr. Britton has not considered it his duty to publish the last written words of Dr. Gray, which were addressed to him upon this subject, and which expressed his positive opinions upon this point.

"There is nothing whatever of an ethical character inherent in a name through any priority of publication or position, which should render it morally obligatory upon anyone to accept one name rather than another; otherwise it would be applicable or true as well in the case of ordinal names, morphological names, teratological, and every other form of name to which now no one feels himself bound to apply the law of priority. The application of this law as at present practised by many botanists, which would make it the one great law of botanical nomenclature before which every other must yield, regardless even of common sense, is a mere form of fetichism exemplified in science. Many instances of the application of this law are not science, but are rather superstition.—*February* 22, 1892."

The letter is short, but to the point. Important as the subject of nomenclature undoubtedly is, there is great danger of over-estimating the importance, and forgetting that Botany is, after all, the science of plants, and not of names; and the incubus of synonymy which is being heaped on the science by the perpetual tinkering of

pseudo-scientists who might be better employed, will prove a serious obstacle to systematic work, and increase the tendency to take up other branches which are not thus hampered, to the neglect of what is by no means the least important aspect of Botany.

It would be extremely interesting to know what were Dr. Gray's "positive opinions on this point," and why Dr. Britton has not found it in his heart to publish them.

Observations on the Scent of Flowers.

In the "Acta Horti Petropolitani," vol. xi., p. 383, Robert Regel gives an account of some observations made at St. Petersburg on the relation of the smell of flowers to external conditions. The relation is an evident one, but is not the same in different plants.

Plants whose flowers smell during the day have a much more intense odour in warm than in cold dull weather, thus the Mignonette and Sweet Pea have a much stronger smell on a hot clear day, and flowers of *Platanthera bifolia* grown in open places are more scented than when sheltered in woods. Flowers of *Reseda odorata* and *Philadelphus coronarius* which have already opened and contain èthereal oil smell strongly in the dark, and do not lose their smell in a dark room until they wither. To ascertain whether the ethereal oil can form in the dark, some pots of *Reseda odorata* and *Matthiola incana*, with quite odourless buds, were put in a dark room, while others in a light room had the inflorescence only cased in boxes covered inside with black paper and thus kept quite dark. The flowers of the second lot opened and smelt perceptibly, though less than when developed normally in a light room, but in the first case only those flowers smelt whose buds were well advanced when put in the dark, the rest had no smell, and no ethereal oil could be detected in sections of their leaves. Hence it is evident that in these two plants ethereal oil can be produced in the dark at the expense of some nutrient substance which, however, can itself only be developed in the light.

The flowers of *Nicotiana longiflora* smell much more at night than by day; they are open at night and in the morning and evening twilight, but closed by day. After a hot sunny summer's day the smell is more intense than after a dull cool day. When kept in darkness they no longer close by day, and smell both day and night. In specimens of which only the inflorescence was enclosed in dark boxes, all the flowers when opened gave a fairly strong smell; even when plants were placed entirely in a dark chamber, it was only in flowers that did not open till after three or four weeks that no smell could be recognised.

Nycterinia capensis behaved somewhat differently. Here also the flowers are closed and have no smell by day, opening only in the twilight and at night, when they are sweetly scented. The smell is

probably due to the presence of Benzolaldehyde (oil of bitter almonds) in the floral leaves, judging not only by the smell but also by chemical tests. After a warm sunny day the smell is stronger than after a dull cooler day. When placed in a dark chamber the plant behaves on the first day just as in a light room, smelling only by night, the flowers being closed by day and open at night. But if, in addition, the temperature be lowered, the result is different; thus, specimens placed during the day in the dark near an ice cellar, at one time at 8° R. and another at 11° R. (instead of the usual day temperature of 14°—20° R.) began to smell perceptibly after an hour, although the flowers were only half open, or even in some cases quite closed. To ascertain the influence of light on the formation of the odoriferous substance, the following experiment was made. Specimens with still odourless buds were placed in the dark chamber—in some cases plants in pots, in others cut-off twigs in water-glasses. Other plants were placed in a light room and the parts of the inflorescence with odourless buds covered with dark boxes. The first flowers opening in these dark boxes smelt evidently, though fainter than normally, and it was only after two or three weeks, when the etiolated inflorescence had grown considerably in length, that no smell could be detected in the opening flowers; those opening earlier continued to smell till they withered.

In the case of plants kept wholly in the dark, the flowers which opened on the first day were scented, but not those opening later, and the former also gradually lost their scent. The single cut-off flowers, or inflorescences, in the water-glasses lost their smell in the dark chamber after three to four days. In every case the complete disappearance of the smell coincided with the complete consumption of the starch in the cells of the floral leaves, only in the stomatal cells could starch-grains be found, and it was possible to foretell with certainty which flowers would open and which would not, by investigating the starch-content of buds in a sufficiently advanced stage of development; the strength of the smell also depended to a certain extent on the quantity of starch in the floral leaves. As a further illustration of this relation, cut-off shoots of the same Nycterinia were placed in the dark chamber—some in distilled water, some in half per cent. sugar solution. After about four days the former had used up their starch and ceased to smell, but the latter did not lose their starch, nor did the flowers cease to smell till completely withered. Even flowers which had already lost their smell in the dark regained it in sugar solution with the formation of starch in their leaves, while shoots taken from the solution and placed in distilled water in the dark lost their smell as soon as the starch disappeared. As the presence of starch is an index of the activity of constructive processes in the plant, the simultaneous disappearance of the smell of the flowers and the starch from the floral leaves of *Nycterinia capensis* shows that the formation of the odoriferous substance stands in the closest connection with the life of the plant.

In this point *Nycterinia capensis* differs from all other scented flowers investigated. In the floral leaves of *Matthiola incana*, *Rosa centifolia*, *Lathyrus odoratus*, *Dianthus plumarius*, *Reseda odorata* and others, either no deposit of starch was found, or only in the stomatal cells, while in the flowers of *Philadelphus coronarius* it occurred only in the stamens. The corolla of *Nicotiana longiflora* is rich in starch-grains, but neither here nor in *Philadelphus* has their consumption the slighest influence on the smell of the flowers; in the latter plant flowers which already smell never entirely lose their scent when placed in the dark.

Experiments were also made on the relation between respiration and the scent in *Nycterinia capensis*. Shoots with the normal night-smelling flowers, when placed in an atmosphere of hydrogen, neither opened nor smelt; the effect was somewhat persistent, as the flowers when removed from the hydrogen did not open for some time, and even then did not smell. The next evening, however, they opened and smelt in the normal way.

Finally, the author points out that although normally only the open flowers of *Nycterinia* are odorous, yet the scent and the opening and closing are not absolutely dependent on each other, for the flowers do not usually begin to smell directly on opening, and we have also seen that under some circumstances, *e.g.*, in the dark, at a temperature lower than the normal, only half-opened or even quite closed flowers may be scented.

Women Fellows.

The Royal Geographical Society has decided to admit women as Fellows. For many years the Society has accepted papers from women, and on two occasions has singled them out for special honour, as when in 1860 Lady Franklin received the Gold Medal in commemoration of her husband's discoveries, and when in 1869—a case more to the point—Mary Somerville received the Patron's Medal for her publications furthering Geographical Science. The *Manchester Guardian* for July 7th has the following excellent comment on the decision:—"Papers by lady travellers have frequently been read before the Society, and it has for some time been felt to be an anomaly that travellers of the standing, say, of Mrs. Bishop (Miss Isabella Bird) should be excluded from the ranks of a Society for which they have done so much good."

While endorsing all this, it must be admitted that there is the strongest objection to the admission of women to scientific meetings as casual visitors; it would probably be advisable to allow only the lady Fellows to introduce ladies, as meetings of a Society held for scientific business are very different from Royal Institution lectures.

We do not remember that any Society has refused to accept a

paper from a woman because she has the misfortune to be a woman, but while accepting her work, most Societies refuse to allow her to be present to hear it read. The Zoological Society's principle and practice on the subject are inconsistent: the former admits women as Fellows, "with the same privileges, and under the same regulations in all respects as gentlemen," but while the rules admit women the doorkeeper excludes them from the meetings! The Geologists' Association still maintains its lead in this matter, for it has this year elected a woman, whose work ranks high, a Vice-President. The Geological Society, however, still refuses women the Fellowship, though it is reported by *Research* (vol. i., 1889, p. 216) that when the subject was last discussed, the reform was only lost by four votes; and, but for the over-confident zeal of the women's advocates, who refused any compromise on the lines suggested above, it would, doubtless, have been carried.

THE Geology of the Bas Boulonnais has received very full description from M. E. Rigaux (*Mem. de la Société Academique de Boulogne*, vol. xiv., 1892). The strata exposed at the surface include Devonian, Carboniferous Limestone, and Coal-measures. Unconformably resting on these are various Jurassic strata, the Great Oolite and Cornbrash, Oxfordian, Corallian, Kimeridgian and Portlandian strata, and then the Purbeck-Wealden. Resting indifferently on Secondary and Palæozoic rocks are representatives of Lower Greensand and Gault (Aptian and Albian). The author describes the strata in detail, and records the fossils from each horizon, so that interesting comparisons can be made with the strata in this and other countries. Some new species of Brachiopoda and Mollusca are described.

WE hear that it is possible that a botanist and geologist may accompany the Eclipse Expedition up the Salum River next April. The evidence for the Cretaceous age of the rocks at Cape Verde does not seem very satisfactory, while nothing is known of the place in the Palæozoic sequence of the older rocks that are supposed to cover a wide extent of country in the Gambia "Hinterland." A very important contribution to the geology of Eastern Africa has just been issued in the last volume of the *Denkschrift der Wiener Akademie der Wissenschaft* of which we hope to say more next month.

THE last number (594) of the *Abstracts of Proceedings of the Geological Society*, contains the result of Professor Seeley's labours on the Saurischia of Europe and Africa. These bird-like reptiles are described as "terrestrial unguiculate ornithomorpha," and the paper discusses the various genera, and their relations and peculiarities. In the discussion which followed, the President, Mr. W. H. Hudleston, remarked "that the paper on Saurischia was received by the officers

of the Society early in April. Since that date Professor Marsh, in his Notes on Triassic Dinosauria (*Amer. Journ. Sci.*, June, 1892), had published, as regards *Zanclodon*, conclusions similar to those at which the author (Professor Seeley) had already arrived."

In the *Scottish Geographical Magazine* for July, Dr. H. Schlichter concludes his paper on the Pigmy Tribes of Africa, and gives a map showing their distribution. The map shows that with the exception of a few scattered races between 15° and 25° S. latitude, the whole of the tribes are confined to the district between 5° N. and 5° S. lat., *i.e.*, to the Gaboon Coast, the forest area of the Congo State, the west side of the Albert Nyanza and the north of Lake Rudolf. The paper is an excellent epitome of our present knowledge of these peoples, and is abundantly supplied with references to other authors. Professor James Geikie's paper, read to the Society on May 18th, on Recent Researches in Pleistocene Climate and Geography, is printed in the same magazine; it is accompanied by a map of Northern Europe, showing the areas covered by ice during the epoch of maximum glaciation, and by the great Baltic glacier and the local ice-sheets of Britain at a later date.

The question of the action of light on the colouring of vertebrate animals is dealt with by Sr. A. L. Herresa in a paper—"El Elima del Valle de México y la biologia de los vertebrados," in *La Naturaleza* (t. ii., c. 2), issued last month. The paper, which partly appeared in the previous number, deals with the production of colour by light reflected by coloured bodies, the modification or production of colours by white light or by reflected light, and terminates with a long series of conclusions and a short paper on the general influence of light. In the same number of *La Naturaleza* there is a paper, "Apuntes para la Geologia del Valle de México," by G. B. y Puga, dealing with the eruptive mass of El Peñón de los Baños, some four kilometres east of the City of Mexico. The hill consists of two roughly semicircular masses, one of volcanic agglomerate, and the other of Retinitic-Andesite, with an included mass of black basalt and an outer fringe of a similar rock. Analyses of the rocks are given, and several illustrations from photographs assist the reader. Some notes are printed by A. Duges on *Dendroica dugesi*, H. K. Coale, the passerine bird described as new in 1887, which show that it should rather be regarded as referable to *D. æstiva* (Gm.). Jose N. Rovirosa contributes a "Calendario botanico de San Juan Bautista y sus Alrededores, año de 1890," which, assisted by a chart, gives the times of flowering and other interesting information, including the local names so often of assistance (or hindrance) in determining plants. A variety, *floribunda*, of *Breweria mexicana*, Hemsley, is described and figured by M. M. Villada.

MR. HALKYARD continues his researches into the Foraminifera of the Channel Islands in the *Transactions of the Manchester Microscopical Society* for 1891. In the present paper, the distribution of the various forms around Guernsey, Herm, and Jersey is attended to, and lists showing their comparative abundance are given. *Carterina spiculolesta*, an interesting find, is recorded from Guernsey and Jersey. Rhizopoda appear a second time in the same publication, for Mr. James Cash has an illustrated paper on the freshwater forms of the Manchester district. M. Fornasini has just published, in *Mem. Accad. Sci. Bologna*, a note on *Nodosaria obliqua* (Linn.), showing the variations of this form. The paper is accompanied by an excellent plate.

DR. BENECHE, the director of the experimental station at Klaten, Java, has, according to *Nature*, offered a prize of 1,000 marks for the best essay on the causes of the red colour in the fibro-vascular bundles of *Sorghum* which accompanies the disease known as "sereh." A similar disease has recently become very destructive to the sugar-cane crop in Java. Dr. Beneche had already published some results of experiments for combating the disease in 1891 ("Proefnemingen ter bestrijding der 'Sereh,' 80, *Semarang*"), and in *De Indische Gids* for December, 1891. Attention is now called to a second paper, published in the "Medederling," of the Middle-Java Experimental Station, entitled "De bestrijding des onder den naam 'sereh' samengevatte ziekteverschijnselen van het suikerriet," dealing more fully with this serious trouble. Further information on this and many other plant diseases and pests will be found in the *Zeitschrift für Pflanzenkrankheiten* (Stuttgart), a journal edited by Dr. Paul Soraner, and which, besides supplying many original articles, collects those published in little-known serials.

BOTANISTS will be glad to hear that Mr. Daydon Jackson's Index to Flowering Plants is in progress of printing by the Oxford University Press. Nearly the whole of the letter B is in type. The passing of the proofs of such a monumental work as this through the Press is extremely arduous, as collation with the whole of the rest of the manuscript is needful for completion. It is difficult to realise the labour involved in such a book of reference.

THE library founded by M. Adrien Dollfus for the benefit of the readers of *La Feuille des Jeunes Naturalists*, grows apace. In fascicule 15 of the Catalogue, just issued, the additions received between January 21 and April 20, 1890, are recorded under numbers 19,233 to 20,519, a total of 1,286 books and pamphlets. The Catalogue is classified under subjects, and the books are available to all subscribers

dwelling in France, on payment of the necessary postage. It is greatly to be hoped that the readers of M. Dollfus' interesting publication avail themselves of the exceptional generosity of the editor in providing this library for them, which, so far as we know, is unique of its kind.

We are glad to see that, among others, the widows of the late Dr. P. H. Carpenter and Mr. G. T. Bettany have been awarded pensions from the Civil List.

The month of July has been remarkable for the succession of catastrophes that have been recorded during it. The most interesting of these is the reported destruction of Sanguir, an island about 130 miles from the northern end of Celebes; according to a Dutch captain this was blown to pieces on the 7th of June by a volcanic explosion, which recalls to mind the famous destruction of Krakatoa. The island covers a little over 100 square miles, and its population is estimated at about 12,000, but this would be a high average for a Malaysian island. If such an explosive eruption has really occurred, it ought to be verified by brilliant sunsets before authentic details can be received.

Etna is now in eruption, and serious damage has been done near Catania, while Vesuvius has also commenced operations, and a strong stream of lava is flowing down the crater into the Atrio del Cavallo, where we hope it will be content to stop. Simultaneous eruptions of Vesuvius and Etna are very unusual, and this adds to the interest with which further progress will be watched. We hope to give a detailed account next month.

Switzerland has been occupying public attention before the usual time, as the season of climbing accidents has barely commenced. The destruction of St. Gervais, however, has caused more deaths in a single night than have all the climbing misadventures of the century. Numerous proposals have been made for the prevention of such disasters in the future, of which the favourite one is a periodical inspection of glaciers; but if the inspection merely results in an order to the glacier to be on its best behaviour, as seems to be the Swiss method of dealing with doubtful boilers, it will not do much to allay uneasiness. The only effective means of preventing such disasters would be to avoid building hotels in narrow gorges, for so many of these bear upon their banks evidence of similar inundations. They have not, however, been often recorded, and the descriptions of the present flood may well replace those of the similar irruption of the Lac de Bagnes down the Dranse valley, which as one of the stock illustrations of geological text books has long been worn threadbare.

I.

The Permanence of the Great Oceanic Basins.

THAT the great Oceanic Basins, as well as the land areas of the globe, have persisted throughout a large portion, if not the whole, of known geological time, is a proposition which has been accepted by writers of such eminence, and is supported by so many distinct lines of evidence, that it seemed likely to become one of the established teachings of geology. Professor Dana was led to it by a study of the development of the North American Continent; Darwin upheld it from his study of Oceanic Islands, and the facts he adduced have since been strengthened by the discovery that the two supposed exceptions to the generalisation that no ancient sedimentary rocks occur on such islands—Rodriguez and St. Paul's Rocks—are no exceptions at all. Two successive heads of the Geological Survey of Great Britain, Sir Andrew Ramsay and Sir Archibald Geikie, have advocated similar views; while Mr. John Murray, of the "Challenger," holds that the vast mass of evidence now accumulated as to the nature of the deposits on the floors of the great oceans, indicates that they are distinct in character and origin from any of the widespread formations which make up the series of the sedimentary rocks. Coming to the subject from a totally different point of view, that of the physicist and mathematician, the Rev. Osmond Fisher arrives at similar results. In the latest issue of his important work, "Physics of the Earth's Crust," he gives as one of his conclusions—"and lastly, that the great oceanic and continental areas have never changed places"; and, in the summary of the whole work, he says: "The occupation of an entire hemisphere by one great ocean is a remarkable circumstance, and we have seen reason for believing that this is a very ancient division of the surface, and that it is probably a mistake to suppose that every part of it has been sometimes raised above the sea, and sometimes depressed beneath it. The truth seems to be, that the region subject to these alterations of conditions does not extend very far away from the present coast lines."

When studying the causes which have brought about the geographical distribution of animals, I was compelled to deal with this question, because I found that it had been the custom of many writers to solve all anomalies of distribution by the creation of hypothetical lands, bridging across the great oceans in various directions

and at many different epochs; and, having arrived at the conclusion that the distribution of organisms could be more harmoniously and consistently explained without such changes of sea and land, which usually created greater difficulties than those they were intended to explain, I gave, in my "Island Life" a brief statement of the evidence which appeared to me to render such changes exceedingly improbable. This evidence was mainly a summary of the facts and arguments adduced by the eminent men referred to above, and to this I added in my "Darwinism" a difficulty founded on mechanical considerations which seemed to me to furnish a preliminary reason why we should not accept the doctrine of the interchange of continental and oceanic areas without very clear and cogent reasons. Since then some other arguments of this nature have occurred to me, and as the theory of permanence has been recently attacked, by Mr. W. T. Blanford in his presidential address to the Geological Society in 1890, and by Mr. Jukes-Browne in his "Building of the British Isles," it may be as well to consider these difficulties, which furnish, in my opinion, a very powerful argument against the interchange of oceanic and continental areas, and which has the advantage of not requiring any knowledge of the higher mathematics in order to estimate its validity.

And, first, it is necessary to clear away some misconceptions as to the proposition I really uphold, since arguments have been adduced which in no way affect that proposition. Thus, Mr. Jukes-Browne quotes Professor Prestwich as saying, "It is only the deeper portions of the great ocean troughs that can claim the high antiquity now advocated for them by many eminent American and English geologists." But this is all that is claimed. For practical purposes I at first took the 1,000-fathom line as, generally and roughly, indicating the separation between the oceanic and the continental areas, because at that time it did accurately divide the continental from the oceanic islands, as defined by a combination of geological and biological characters. It has, however, been since shown that two ancient continental islands—Madagascar and New Zealand—are separated from their respective continents by depths of more than 1,000 fathoms. We must, therefore, go as far as the 1,500-, or, perhaps, in a few cases, to the 2,000-fathom line, and this will surely mark out "the deeper portions of the great ocean basins," since only isolated areas exceed 3,000 fathoms.

Now, if we look at the deep ocean basins marked out by the 2,000-fathom line, not on Mercator's projection which greatly exaggerates the shallower portion situated in the temperate and polar regions, but on an equal-area projection, such as the map which illustrates Mr. J. Murray's paper,[1] we shall see that by far the larger part of all the great oceans are included by this line, and that, for

[1] "On the Height of the Land and the Depth of the Ocean," *Scottish Geographical Magazine*, 1888.

the purpose of indicating the isolation of the continents from each other throughout the equatorial and most of the temperate zones, there is very little to choose between the 1,000-fathom or the 2,000-fathom boundary. The latter, however, allows more scope for possible land extensions between the three southern continents and the Antarctic lands, which, during mild epochs, and by means of intervening islands, may, perhaps, have served as a means of communication between these continents. All that is necessary to maintain, therefore, is that existing continents with their included seas, and their surrounding oceanic waters as far as the 1,500-fathom, or in some extreme cases the 2,000-fathom line, mark out the areas within which the continental lands of the globe have been built up; while the oceanic areas beyond the 2,000-fathom line, constituting, according to Mr. Murray's data, 71 per cent. of the whole ocean, have almost certainly been ocean throughout all known geological time.[2]

It will now be seen that this is a problem which deals with the very broadest contrasts of the earth's surface, and that its fundamental data are on so vast a scale as not to be materially affected by the smaller details of physical geography, or by differences of opinion as to the exact meaning of certain terms. Whether a particular island is more correctly classed as oceanic or continental, whether a certain portion of the ocean should be placed within the oceanic or the continental area, and whether certain rocks were formed in very deep or in comparatively shallow water, are of slight importance, except in so far as they may throw light on the real question, which is, whether the vast expanses of ocean beyond the 1,000-fathom line, forming about 92 per cent. of the whole oceanic area, have ever been occupied, or extensively bridged over, by continental land. It is towards the solution of this great problem that I now propose to submit certain general considerations which appear to me to lie at the root of the whole matter.

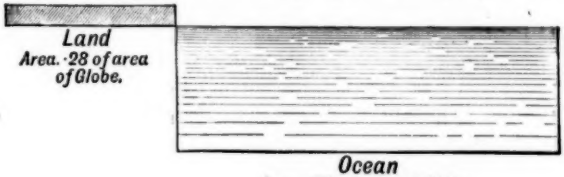
Comparison of Oceanic and Continental Masses.—In the paper already referred to, Mr. John Murray has carefully estimated both the area of the land and of the water on the earth's surface, and their bulk as deduced from the best available data. Taking the whole area of the globe as 100, he finds the land surface to be 28, the water surface 72. But the mean height of the land above sea-level is 2,250 feet, while the mean depth of all the oceans and seas is 12,456 feet. In this estimate, however, all the inland seas and shallow coast waters are included, and as these, at least as far as the 100-fathom line, are universally admitted to be within the "continental area," we omit them in our estimate of the mean depth of the oceans proper, which are thus brought to something over 15,000 feet, or nearly seven times as much as the mean height of the land.

[2] Mr. Murray gives his results for oceans and inland seas together. The above refers to the oceans only as usually understood; but the difference is not great.

The accompanying diagram (taken from my book on "Darwinism") will better enable the reader to appreciate these proportions, which are of vital importance in the problem under discussion. The lengths of the two parts of the diagram are in proportion to the areas of land and ocean respectively, the vertical dimensions showing the comparative mean height and depth. It follows that the areas of the two shaded portions are proportional to the bulk of the continents and oceans respectively.

The mean depths of the several oceans and the mean heights of the several continents do not differ enough from each other to render this diagram a very inaccurate representation of the proportion between any of the continents and their adjacent oceans; and it will therefore serve, roughly, to keep before the mind what must have taken place if oceanic and continental areas had ever changed places. It will, I presume, be admitted that, on any large scale, elevation and subsidence must nearly balance each other, and, thus, in order that any area of continental magnitude should rise from the ocean floor till it formed fairly elevated dry land, some corresponding area must sink

Diagram of proportionate mean height of Land and depth of Oceans.



to a like extent. But if such subsiding area formed a part or the whole of a continent, the land would entirely disappear beneath the waters of the ocean (except a few mountain peaks) long before the corresponding part of the ocean floor had approached the surface. In order, therefore, to make any such interchange possible, without the total disappearance of the greater portion of the subsiding continent before the new one had appeared to take its place, we must make some arbitrary assumptions. We must suppose either that when one portion of the ocean floor rose, some other part of that floor sank to greater depths till the new continent approached the surface, or, that the sinking of a whole continent was balanced by the rising of a comparatively small area of the ocean floor. Of course, either of these assumed changes are conceivable and, perhaps, possible; but it seems to me that they are exceedingly improbable, and that to assume that they have occurred again and again, as part of the regular course of the earth's history, leads us into enormous difficulties. Consider, for a moment, what would be implied by the building up of a continent the size of Africa from the mean depth of the ocean. By

comparing the area of Africa with that of the whole of the land, and the depth of the ocean with the mean height of the land, we shall find that if all the land of the globe above sea-level could be transferred to mid-ocean, it would not be sufficient to form the new continent, but would still leave it nearly 2,000 feet beneath the surface.

It thus appears that, if the elevation of the ocean floor, and the corresponding sinking of whole continents, constitute a portion of the regular change and development of the earth's surface, there would be not only a chance but a great probability of entire continents disappearing beneath the waters before new continents had risen to take their place. Even the total disappearance of all the large land masses might easily happen; for we see from the diagram that they might one after the other disappear with a corresponding rise of the adjacent portion of the ocean bed and still leave the ocean over the whole earth almost as deep as it is now. But, as will be shown further on, the geological record, imperfect as it is, teaches us that no such submergences have ever taken place.

Contour of the Ocean Floor as indicating Permanence.—Before extensive soundings revealed the depth of the ocean and the form of its floor, it was supposed that it would exhibit irregularities corresponding to those of the land, such as mountain-ranges, great valleys, escarpments, ravines, &c. But we now know that the main characteristic of the ocean floor is, that it is a vast undulating plain, the slopes rarely exceeding a hundred feet in a mile except near the margins of the continental areas, while usually the gradients are so slight that they would be hardly perceptible. Contrast this with the forms of all mountain ranges whose general rise for long distances is often five hundred feet in a mile, while slopes at angles of from 20° to 60° are by no means uncommon.

Now if we suppose that considerable portions of the ocean depths have been formed by the subsidence of continents, we should certainly expect to find some indication of those surface features which characterise all continents, but which appear to be absent from all deep oceans. In order to account for the actual contours of the ocean on this theory, we must suppose that, during subsidence, all the mountain ranges, peaks, valleys, and precipices were reduced to an almost uniform level surface by marine denudation, which, unless the process of subsidence were incredibly slow, seems most improbable. Mr. Jukes-Browne, however, does not hold the view that they have been thus denuded, for he approvingly quotes Mr. Crosby as saying that—"the oceanic islands are, of course, merely the tops of submerged mountains, and it is only with the highest points of continents that they can properly be compared." If this is correct, then we ought to find in the vicinity of such islands all the chief features of submerged mountain ranges—precipices, deep valleys and ravines, arranged in diverging groups as they always

occur in nature. But in no single case, that I am aware of, have any such features been discovered.

But a still greater difficulty remains to be considered. If oceanic and continental areas are interchangeable, it can only be because the very same causes (whatever they may be) that produce elevations and subsidences in the one produce them also in the other, and at first sight it appears probable that this would be the case. But if these causes have been at work upon the ocean floor throughout geological epochs, they would have produced irregularities of surface not less but far greater than on subaerial land. This must be so, because subaerial denudation continually neutralises much of the effect of upheaval in the continental areas, while in the ocean depths no such cause or anything analogous to it is in operation.

The forces which have been at work in every mountainous region have sometimes tilted up great masses of rock at high angles, upheaved them into huge anticlinal curves, or crushed them by lateral pressure into repeated folds, which in some cases appear to have fallen over so as to reverse the succession of the strata. But, notwithstanding these various forms of upheaval, involving vertical displacements which are sometimes several miles in extent, the surface of the land often shows no corresponding irregularities, owing to the fact that subaerial denudation has either kept pace with upheaval or has even exceeded it, so that the position of an anticlinal ridge may be, and often is, represented by a valley. Now, if we suppose that similar forces have been at work on that portion of the earth's surface forming the bed of the ocean, where there are no such counteracting agencies, we should expect to find irregularities in the ocean floor far greater than those which occur upon the land surface.

Still more difficult to explain would be the absence of precipitous escarpments due to faults, which, though frequently showing an upthrow or downthrow of the strata to the amount of many hundreds and sometimes thousands of feet, rarely exhibit any difference of level on the land surface, owing to the fact that subaerial denudation has kept pace with slow and intermittent elevation. But in the ocean depths no such denudation is going on; and we can therefore only account for its very uniform surface on the supposition that it is not subject to the varied and complex subterranean movements which have certainly acted within the continental areas throughout known geological time.[3]

Equal Range of the Geological Record in all the Continents.—There is one other general consideration which indicates the permanence and continuity of the Continental Areas, and which renders it very diffi-

[3] The Rev. O. Fisher has arrived at this conclusion from his own researches. He says: "The compression which has caused the thickening accompanied by corrugation, such as characterises most elevated tracts, is properly a continental phenomenon, and has no analogue beneath the oceans." L.c., p. 253.

cult, if not impossible, to suppose that they have ever changed places with the great oceans. It is, that on all the present continents we find either the same or a closely parallel series of geological formations, from the most ancient to the most recent. Not only do we find Palæozoic, Mesozoic, and Caniozoic rocks everywhere present, but, in proportion as the continents are explored geologically, we find a tolerably complete series of the chief formations. From Laurentian to Carboniferous and Permian, from Trias to Cretaceous, and from Eocene to Quaternary, the geological series appears to be fairly represented not only in continents, but also to a considerable extent in the large continental islands, such as Great Britain and New Zealand.

Now this is certainly not what we should expect if the present continental areas had, at different epochs, risen out of the deep oceans. In that case some would have commenced their geological history at a later period than others, having either a late Palæozoic or some Mesozoic formation, or even an early Tertiary for their lowest stratified rock. Others, which had become oceanic for the first time at a later epoch, would exhibit an enormous gap in the series, either several of the Mesozoic formations, for example, being absent, or some considerable portion of both Palæozoic and Mesozoic, or Mesozoic and Tertiary. This would necessarily be the case, because we cannot believe that so vast a change as the subsidence of an entire continent till its site became a deep ocean, and its subsequent elevation till it again became dry land, could possibly be effected in any less extended periods, if at all.

Whenever such gaps, or smaller ones, now occur locally, they are generally held to imply the existence of terrestrial conditions, as in the case of China, which, according to Richthoven, has been continental since the Carboniferous epoch. In many cases there is distinct evidence of such conditions in lacustrine or freshwater deposits, dirt-beds, &c. But if a gap of such vast extent both in space and time as that here referred to were caused by the interchange of a continent and a deep ocean, the fact that it was so produced would be clearly evidenced by an almost uniform deposit either of organic or clayey ooze, similar to those now everywhere forming over the oceanic area. Even if we make the fullest allowance for denudation during elevation, sufficient indications of so widespread a formation should be detected. Such a deposit would, in fact, have every chance of being largely preserved, because, long before it rose to the level where it would be subject to denudation by waves or currents, it would, almost everywhere, be overlain by a series of shore deposits, and wherever these latter were preserved on the land surface the oceanic formation would necessarily be found under them. That no such enormous deficiency in the geological series characterises any of the continents, and that no widespread deposit of organic or clayey ooze at some

definite horizon has been anywhere detected, though such a deposit must have been formed and largely preserved if the whole or any considerable part of a continent had risen from ocean depths at any period of its history, constitutes of itself a very strong argument against any such interchange of oceanic and continental areas having occurred.

I have now shown that there are three distinct groups of phenomena which are either altogether inconsistent with any general interchange of oceanic and continental areas, or which render it exceedingly difficult to understand how such interchange could have been brought about. These phenomena are:—(1) The enormous disproportion between the mean height of the land and the mean depth of the ocean, which would render it very difficult for new land to reach the surface till long after the total submergence of the sinking continent. (2) The wonderful uniformity of level over by far the greater part of the ocean floor, which indicates that it is not subject to the same disturbing agencies which throughout all geological time have been creating irregularities in the land-surface, irregularities which would be far greater than they are were they not continually counteracted by the lowering and equalising effects of subaerial denudation. (3) The remarkable parallelism and completeness of the series of geological formations in all the best known continents and larger continental islands, indicating that none of them have risen from the ocean floor during any portion of known geological history, a conclusion enforced by the absence from any of them of that general deposit of oceanic ooze at some definite horizon, which would be at once the result and proof of any such tremendous episode in their past history.

I submit that these facts, and the conclusions to be logically deduced from them, form a very powerful *à priori* argument as against those who maintain the interchange of continents and oceans as a means of explaining certain isolated geological or biological phenomena; such, for instance, as the much-disputed origin of the chalk, or the supposed necessity for land-communication to explain the distribution of certain groups of reptiles or fishes in remote geological times. Before postulating such vast revolutions of the terrestrial surface in order to cut the gordian knot of difficulties which may be mainly due to imperfect knowledge, it will be necessary to show that the considerations here adduced, as well as the great body of facts which have caused many eminent geologists, naturalists, and physicists to hold the doctrine of oceanic permanence, are either illogical or founded on incorrect data. For, surely, no one will suggest that so vast a problem of terrestrial physics can be held to be solved till we have exhausted all the evidence at our command, and have shown that it largely preponderates on one side or the other.

The present article is intended to supply a hitherto unnoticed class of arguments for oceanic permanence, and these must of course

be taken in connection with the other evidence which has been summarised in the 6th chapter of the writer's "Island Life," and also with the admirable "Summary" of the purely physical argument in the 2nd edition of Mr. O. Fisher's "Physics of the Earth's Crust." It is certainly a remarkable fact that writers approaching the subject from so many distinct points of view—as have Professor Dana, Mr. Darwin, Sir Archibald Geikie, Dr. John Murray, Rev. O. Fisher, and myself—should yet arrive at what is substantially an identical conclusion; and this must certainly be held to afford a strong presumption that that conclusion is a correct one.

ALFRED RUSSEL WALLACE.

II.

The Climate of Europe during the Glacial Epoch.

CLIMATIC changes in byegone times may account for many peculiarities in the present geographical distribution of animals and plants. The last "Glacial Epoch," however, has been held responsible for so many anomalies, that it is not surprising to find the more cautious naturalist somewhat chary of referring anything to this cause. Still, it is impossible to doubt that important changes of climate have taken place within the lifetime of existing species, and that these climatic changes necessarily led to much extermination and forced migration. Evidence of this may be found, no doubt, in the present distribution of animals and plants; but clearly to understand the origin of the existing faunas and floras, we must first master the outlines of their former distribution. We must learn, also, what climatic and orographic changes have taken place, and what parts of the world these changes affected.

All this may seem self-evident. But though biologists now understand that embryology is essential, and that the study of extinct forms cannot be neglected, yet the writer on geographical distribution is often content to speculate on what may have been, without reference to the geological evidence as to what really did happen. It is as if we read our history, like a witch's prayer, backwards; or as if, in writing a biography of a celebrated man, we commenced with his funeral, or described nothing but his last moments. Such a method of studying history would be absurd; but it is the method often adopted in natural history.

A rapidly increasing body of naturalists recognises, however, that there is an evolution of faunas and floras to be studied, as well as an evolution of species. For the benefit of these naturalists it may be useful to summarise the present state of our knowledge of the later Tertiary changes in Western Europe. This I venture to do; but it must be confessed that we are as yet merely commencing a very large enquiry, and that unexpected and often puzzling pieces of evidence are met with daily. We shall ultimately, I hope, be able to write a fairly continuous epitome of the most recent chapter in the earth's history; at present we can only deal with more or less

detached episodes. Yet, as with casual finds of ancient documents, a study of these episodes will often throw light on what happened much later, though the intermediate history may still be a blank.

The first question here to be dealt with will be, What was the temperature in Britain during the Glacial Epoch, and how far did the cold affect more southern regions? In discussing this matter, I do not propose in any way to speculate on the cause of the climatic change; under the present circumstances it is, perhaps, better to treat the question simply as one of evidence. There are several theories in the field; if one of them can be made to fit the facts, well and good, but we must not allow any preconceived theory to bias us in this enquiry.

When we attempt to learn what climatic conditions held during past times, several different classes of evidence are available. We can treat the question as a purely physical one; we can treat it from a botanical point of view; or from a zoological. Finally, we can combine the independent chains of evidence, and if they all lead to the same conclusion we may consider that we have established a safe working hypothesis.

To the naturalist the most interesting part of the enquiry will probably be, What was the lowest annual temperature met with during the Glacial Epoch? This question, till lately, would have been difficult to answer with any confidence, but recent advances in our knowledge of the present condition of the Arctic Regions, and recent studies in Pleistocene geology, make a fairly accurate estimate quite possible.

The temperature of the sea and of the air do not necessarily correspond in the same regions. We will, therefore, first discuss the evidence as to the lowest temperature of the seas around Britain. For this purpose the former southern limit of floating ice, and the southern limit of the formation of shore ice, or "ice-foot," ought to give a fairly accurate idea as to the temperature of the water. No doubt a large iceberg may travel a long distance through comparatively warm water before it entirely melts away; but shore ice, such as forms every winter in the Arctic Regions, must float in water nearly as cold as itself if it is to carry its burden of beach-stones and erratic blocks any great distance.

The English Channel, during the greatest severity of the Glacial Epoch, seems to have occupied the southern limit of the formation of ice-foot—or, at any rate, of shore-ice sufficiently thick to transport anything but small stones. Ice-foot such as collects every winter in the Arctic Regions then fringed our south coast, and beset the shores of Brittany and of the Channel Islands. When, in the spring, this ice became detached it transported its burden of included rocks hither and thither, even across the Channel. We thus find at Selsea Bill, in Sussex, erratics weighing several tons, but undoubtedly transported from Bognor or from the Isle of Wight. Others, equally large, have

probably come from the Channel Islands and the coast of Brittany; one block of granite is probably from Cornwall. This fact of the transportation of large erratic blocks for distances of at least a hundred miles, shows that the temperature of the water in the spring, though sufficiently high to dislodge the ice, was yet too low to melt it rapidly. Even with a strong wind a flat mass of shore-ice would take several days to cross the Channel.

To compare this ancient ice-laden English Channel with existing seas, it is necessary to travel northward, till we cross the isotherm of 32° F., and are near the Arctic Circle. The English Channel, being almost land-locked, no cold current like that of Labrador can have had much influence on the temperature of the water. It is possible, however, that the Channel was then much less salt and froze more readily, but even in that case the winter temperature can scarcely have been less than 20° lower than at present.

Turning next to the evidence yielded by the marine fossils, we are still unacquainted with the arctic species of the south of England. The deposits which probably contain them are, unfortunately, beneath the sea-level, and we are unable, till a marine fauna is obtained, to speak confidently of the temperature of the water, or to say whether it was salt or brackish. The shores of the North Sea, however, yield much more satisfactory evidence; for at Bridlington, in Yorkshire, the Boulder Clay contains some included shelly masses with the most intensely arctic marine fauna yet found in Europe. Indeed, this fauna is as thoroughly arctic as any now living in the Arctic Regions, and must have flourished in a sea whose temperature was little above the freezing-point. The fauna of the "Bridlington Crag" does not, however, mark the maximum intensity of the cold, for the Till in which these shelly masses are embedded indicates a still lower temperature, which caused solid ice to fill the whole of the North Sea, and to plough up the marine clays previously deposited. As far as the evidence will allow us to judge, the North Sea, then as now, was a few degrees colder than the English Channel. No trace of ancient glaciers has yet been found on the shores of the Channel.

Carrying our investigations further south, we find in France few indications as to the temperature of the sea during any part of the Glacial Epoch. In the Mediterranean region, however, certain upraised sea-bottoms on the flanks of Mount Etna yield the evidence for which we are searching, for they indicate by their included fossils a temperature a few degrees—perhaps 5°—lower than that of the present sea. Of course, it is not certain that these Sicilian deposits were laid down during the greatest intensity of the cold; still the series of Pliocene and Pleistocene strata seems there to be complete, and careful search has not led to the discovery of any fossil fauna of more boreal character in other parts of the Mediterranean basin. If no indications of greater cold are to be found in the ancient marine

deposits of the Mediterranean, we may, therefore, consider that the severity of the Glacial Epoch died out rapidly towards the south, a refrigeration of 20° in Britain being represented by one of 5° only in the South of Europe.

Thus far we have dealt solely with the temperature of the sea. We will now study the evidence as to the temperature of the air during the same period, and here again we can employ both physical and biological data. The evidence for the former existence in Northern and Central Europe of vast sheets of ice, of greatly extended glaciers, and of deeply frozen soil, is overwhelming, and is so familiar that it need not again be discussed. All the country north of the Thames, and north of a line drawn eastward through Saxony, was buried under a perennial sheet of ice and snow. Near this limit, and apparently advancing or retreating with the ice, stretched a belt of barren and frozen land, with dwarf birch and willow and other arctic plants, but without trees. In this belt flourished also a mammalian fauna like that now inhabiting the Arctic Regions.

Remains of this boreal fauna and flora have now been found so abundantly, and the list includes so many species, that it would be impossible in the limits of a short article to give more than one or two examples. Taking the examples from what is now the warmest part of our islands, we may instance the dwarf birch and the bear-berry found in clays at Bovey Tracey, in Devonshire, though both these species are arctic and alpine plants not now living in the South of England. In several parts of Norfolk and Suffolk a still more arctic flora is found, for it includes the *Salix polaris*, a plant now extinct in Britain, even on the highest mountains. Arctic mammals, also, must formerly have been abundant in the South of England, for besides the lemmings and musk-ox of the Thames Valley, we have at Fisherton, near Salisbury, quite an extensive arctic fauna.

An arctic flora like that just described has lately been traced through great part of Central and Northern Europe by Professor Nathorst; and the corresponding mammalian fauna, with the interesting addition of certain species now inhabiting the cold deserts of Central Asia, is gradually being brought to light, principally through the exertions of Professor Nehring.

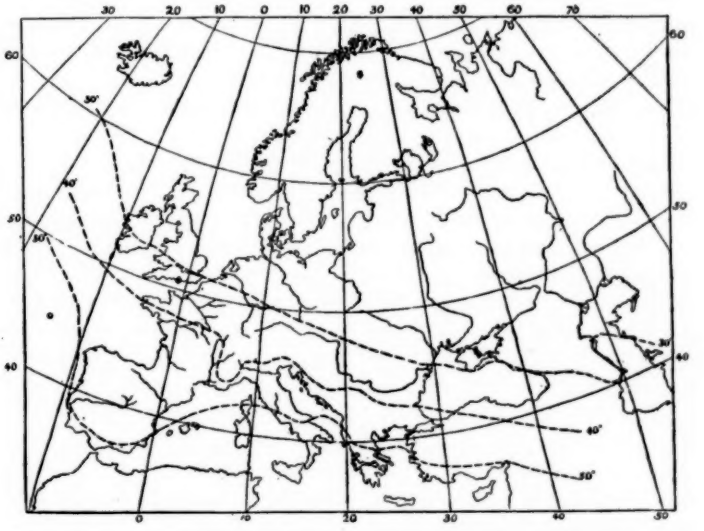
To compare then with now, we must again go to high northern regions, for it is only within the Arctic Circle that we can find physical conditions, and a fauna and flora, like those which once characterised our islands. Britain in those days was probably much like the existing Greenland, though with important differences caused by geographical conditions. After making every allowance for the probable excessive snowfall in Western Europe, it seems impossible to place the average temperature at the southern margin of the ice-sheet higher than the freezing-point—that is to say, 20° lower than at present. It must be remembered that we are dealing mainly with a low flat country, not much above the sea-level, and distant

from any mountains. If the temperature had been much above the freezing-point the ice would melt before reaching London. Even an excessive snowfall would scarcely allow the ice to travel so far unmelted, unless the air were very cold.

The arctic plants and animals found fossil in Britain point to a similar conclusion, for the same species now flourish in a climate fully 20° colder than that of the lowlands of Britain.

Further south, though the Alps, Pyrenees, and Caucasus show evidence of a former greater extension of the glaciers, yet, when we

ISOTHERMAL MAP, SHOWING THE PROBABLE MEAN ANNUAL TEMPERATURE (FAHRENHEIT) DURING THE GREATEST SEVERITY OF THE GLACIAL EPOCH.



NOTE.—No attempt has been made to restore the ancient topography; as far as yet known there was little change in the boundaries of sea and land during the Glacial Epoch.

remember that extensive snowfields must cause fogs and greater condensation locally, it does not appear that the climate in the lowlands of Southern Europe can have been so much as 20° below the present mean; 10°, or perhaps less, seems more likely to have been the extent of the refrigeration in the Mediterranean region. Though the reindeer was once abundant in Central France, few remains of northern mammals have yet been found near the Mediterranean.

All the evidence yet available seems to show that during the Glacial Epoch the temperature increased rapidly towards the south, and that the effects of the cold did not extend very far towards the equator. Future discoveries in North Africa may necessitate a modifi-

cation of these views; but it should not be forgotten that the northern shores of the Mediterranean have been well explored geologically, and that Italy and Sicily especially have long been classic ground for the student of Pleistocene geology.

To indicate more clearly the probable condition of Europe during the greatest intensity of the cold, I have ventured to construct an isothermal map from the data now available. Of course, the isotherms are mere rude approximations, to be corrected and improved as materials accumulate; but for the purpose of the student of natural history, and especially of geographical distribution, I think that the map may be useful. It will show at least that extensive regions must have been quite uninhabitable for their present fauna and flora. In other regions the crowding of the isotherms would probably bring into competition species that seldom meet under present conditions; and this crowding may help to account for some of the anomalies in the occurrence of the Pleistocene mammalia. A great deal of the supposed discordance in the evidence results, however, from careless observation, and from the deposition of already fossilised bones among others of later date. My own observations have shown few genuine cases of discordant evidence as to climate among the mammals, and none at all in other groups.

An objection, apparently fatal, will occur to any naturalist who attempts to verify the conclusions above arrived at. He will remark that surely in all the thousands of years that have passed since the origin of existing species, many forms must have become acclimatised, and if the physical change were sufficiently slow the animals and plants would accommodate themselves to the new conditions. Had this question been put a few years since I should have felt that the objection was perfectly valid; but one of the most unexpected results of the study of later Tertiary natural history has been to show that, in a wild state, species seldom become acclimatised. They are exterminated, or die out locally, or, perhaps, in a few cases, change into other so-called species, with each climatic change; but every recurrence of cold or warm conditions causes the reappearance of the appropriate fauna and flora.

Only one phase of the Glacial Epoch, the lowest temperature reached, has been dealt with in these notes. It still remains to speak of interglacial mild episodes, of non-glaciated areas surrounded by ice ("Nunataks," as they are called in Greenland), of arid episodes, and of the extremely hypothetical "Pluvial Period." These subjects can, perhaps, be better treated of in a separate article, but it may be advisable to give a word of caution to naturalists who are inclined to treat the severity of the cold and the accumulation of snow as the only climatic factors to be taken into account. Recent observations have shown more and more clearly that throughout Central Europe there was also a period of *dry* cold, causing that region to resemble the arid wastes of Central Asia.

These desert conditions seem to have extended, in a modified degree, into the South of England. In certain deposits we find remains of various locally-extinct species which seem to have nothing in common among themselves, except that they all now live in sunnier climes than ours.

CLEMENT REID.

III.

Death.

THIS makes no pretension of being an elaborately thought-out essay on "Death." It is the result of an attempt to clear up my own ideas on a subject which, when vitalistic conceptions are spreading so rapidly among biologists, seems to be a last outpost of the other school, and yet an outpost which may be the centre of its victory.

I propose to offer no historical introduction; for just as nothing so entirely unfits one to understand current political problems as what is called the study of history, so, however interesting to the curious, the past history of any scientific controversy is the most misleading of all guides to its subsequent development.

In biology, as in morals, simplicity of term invariably covers confusion of idea. The apparent opposites life and death, like the apparent opposites right and wrong, extend as a vast tangle of intermeshing ideas. As out of a naïve acceptance of right and wrong as simple ideas come the cruel follies of the conscientious, so out of a naïve acceptance of the simple words life and death as corresponding to simple facts comes the grotesque abstraction—vitalism.

There are three ways in which an organism may come to an end. By some gross accident of life, as when one cleans an amœba off a slide with a duster, or oneself trifles with nitro-glycerine, an organism may be swept out of existence. Or, and specially in the case of the higher organisms, some alteration in the machinery, some slight defect in the mechanism, may interfere fatally with the whole. Or, lastly, the wrought iron itself may slowly oxidise, or by constant vibration become crystalline, the cells and protoplasm of the living organism may decay. Of course, the divisions of this classification, like those of all classifications that do not deal with beans or buttons, or the subject matter of metaphysics, are not mutually exclusive. They represent only the point of view for the moment convenient to the classifier.

With the first mode of death, though it probably enormously preponderates in the animal world—so enormously as to make it doubtful whether natural selection can have anything to do with fixing the term of natural life—we have at present little to do.

The individual life has been destroyed by a mechanical force: the potential energy of the organism provides food for luckier organisms, and sooner or later passes from the world as heat. But in the large organism, as in the small, what has taken place is mechanical interruption of a complicated process, resulting ultimately in chemical death—the running off of the potential energy.

The leading idea in this paper is to find in the various phenomena grouped under the term death, never the opposition of a kindred mysterious force to the mysterious vitalism, but the action of known mechanical or chemical agencies, and to suggest the inference that the life of the organism or of the cell, coming to an end as it does by known agencies, cannot itself depend in essence on any transcendental force; that, in fact, as the negative conditions of life are purely chemical and physical, life itself, however complicated, must be a congeries of phenomena ultimately to be resolved into purely chemical and physical forces.

When we consider the higher animals we find that death is always more or less violent. As Michael Foster says, "The central factor of life is the circulation of the blood, and the blood is not only useless but injurious unless it be duly oxygenated, and in the higher animals the oxygenation of the blood can be duly effected by means of the respiratory muscular mechanism presided over by the respiratory centre in the bulb. Thus, the life of a complex animal is, when reduced to a simple form, composed of three factors—the maintenance of the circulation, the access of air to the hæmoglobin of the blood and the functional activity of the respiratory centre, and death may come from the arrest of any one of these three. As it has been put, death may begin at the heart, or the lungs, or the brain."

It requires little medical knowledge to refer the final symptoms of any fatal disease to one of these—the three gates of death of the older writers. Turning over a few of them, one finds the proximate causes of death to group themselves simply.

Death through the heart may come by inhibition, as when a blow on the abdomen or iced milk on the stomach stimulates the peripheral ends of the vagus. Or it may come by cardiac failure as in fatty degeneration, or in valvular diseases.

Death through the lungs may come as asphyxia from lung diseases, from capillary bronchitis, or from pneumonia. Or asphyxia may result from anæmia caused by loss of blood from hæmorrhage or want of hæmoglobin. Or the respiratory nerves may be paralysed, as in diphtheria or bulbar paralysis.

And, lastly, death through the brain may come in a multitude of ways. Centres may be suddenly destroyed by wounds, or by the culmination of tumours, or they may be paralysed by high temperature, or by a multitude of organic poisons arising from organic diseases, like Bright's, or produced by parasitic bacteria.

Even in death from old age we find the dark mother throwing

open one of the three gates. The frame of a higher animal does not, like the autocrat's one-horse shay, slowly and equally decay until there is a simultaneous collapse of everything—until each cell is senile and moribund—but the heart or the lungs, or some centre in the brain suddenly gives way. But here, perhaps, more plainly than in other cases, we see that the violence or suddenness of death, on which so much stress has been laid, is the merest accident, due to the complexity of the organism. There has been going on for long a gradual degeneration of the walls of the blood-vessels—a degeneration showing itself by an accumulation of fat or of lime—products usually oxydised or excreted, and this slow cellular change has, by weakening the circulation, acted on all the organs of the body, and reacted on itself until some point is reached at which the gross mechanism breaks down. So, in the cases of most causes of death, we can trace back the chain of events until we find that what is said to cause death is only an accidentally last term in a long series of protoplasmic changes; changes, as in lung diseases, where the lung cells have been gradually decaying, or, as in Bright's disease, where kidney cells have been failing to do their work.

In short, protoplasmic changes have been going on, and when the results of these have sufficiently accumulated, the gross mechanism is suddenly arrested; or, more directly, an interference may result when some natural mechanism is accidentally overstimulated, as when reflex stimulation of the vagus stops the heart, or when a lethal alternating current is switched through the spinal cord; or again, a poison may paralyse a brain centre, and so the heart or the lungs be stopped.

It is clear that in all these cases—the modes of death treated of in the medical books—a confusion of ideas exists. The source of this confusion is clearly seen when one finds, for instance, Foster suggesting that the point of death cannot be determined, as it depends on the skill of the experimenter endeavouring to resuscitate a flickering vitality; or Weismann and Gotte gravely discussing whether there can be death without a corpse, and whether death is a subtle attribute of the metazoa teleologically built up by natural selection into its present form. We have to distinguish between protoplasmic death, and the stoppage of a complicated organism.

We know with absolute certainty that whatever be the details of the processes going on in a living cell, the sum of the activity is the acquisition of potential energy either indirectly by the transformation of kinetic energy, as in green plants, or directly by the absorption of chemical compounds possessing high heat equivalents—I mean compounds which, if oxydised in a calorimeter, melt relatively high quantities of ice: the change of these chemical compounds into substances that can melt relatively smaller amounts of ice. The difference of heat equivalent represents the work done by the cell, and the waste in doing the work. Protoplasmic death is an

arrest of this process, an arrest causing in the first place a diminution or cessation of absorption of fresh food—of more potential; and in the second, an increase in the heat waste, and consequent lowering in the heat equivalents of the discharged products. This lowering may be rapid, as where the cell tissues are rapidly oxydised in the air, or it may be a slow process, gradually going on in the bodies of animals or tissues of plants. But the essence of protoplasmic death is that the self-feeding gear is disarranged, that the cell either stops taking up more food, or that it uses up its food more quickly than it can take it, and so burns itself away.

In a complicated aggregate of cells, protoplasmic death of any one of them must necessarily be reflected on the others in some mechanical way, the simplicity of the path depending on the simplicity of the cell aggregate. The suddenness or violence of the interference will similarly depend on the degree of complexity of the cell aggregate. In an organism like man the arrest of the cell activity of the respiratory centre causes what is called sudden death of the entire organism. But obviously the suddenness, as the death itself, depends on the correlations between the organs—correlations of clearly mechanical nature—and the death of the entire organism, protoplasmic death of all the cells—comes only gradually, as the destructive changes set going creep along the mechanical channels. There is, of course, absolutely no reason why the skin of a man killed by chloroform or gunshot should not succeed as a graft quite as well as the skin from a living man, provided it were taken before the ripples of nutritive and other disturbance had spread to it. The tissues die slowly, and the time of death varies as the mechanical relations.

It is clear, then, that death of the organism, the death of the medical books and the popular imagination, is a function, not of the organism but of the complexity of the organism. It forms a mechanical middle term between protoplasmic death of some of the tissues and protoplasmic death of all the tissues. We may distinguish this as somatic death.

Running down the animal kingdom from the more integrated to the less integrated, we find somatic death gradually fading away till, in the protozoa, there is no distinction between somatic death and protoplasmic death. A shock that is fatal to civilised man is not infrequently borne with comparative ease by savage man, and might have little effect even on one of the higher mammalia. Brain operations and accidents instantly fatal to man can be survived for many days by pigeons, and the removal of great parts has little effect on the life of an amphibian.

Precisely the same condition of things can be inferred if we study the results of artificial section, or the conditions of natural budding. Bisection certainly would kill all the higher animals. In its natural form it becomes an increasingly common form of reproduction, as we

descend the animal scale. Moreover, in the embryonic stages of the higher animals—in those stages where integration is less complete—fission as an internal or as an external accident produces, not death, but double monsters or identical twins. So, also, if there be considered the suddenness of somatic death. The heart of a higher animal stops suddenly, and this accidental moment is named the moment of death. But the heart of a tortoise that has been in a dissecting dish for two or three days, begins to beat as the knife reaches it. The watchers by the death-bed of a fish or a mollusc would continue to watch until putrefaction was obvious. I have fed a semi-putrid anemone with pieces of mackerel, and have seen a star-fish with the tips of its arms rotten and pulpy crawl gaily about and evert its stomach on a pecten. In both of these cases the animals were moribund, and soon afterwards actually died. It is a commonplace of observation how rapidly the lower animals putrefy, and how necessary, to secure their histological preservation, it is to kill them as soon as possible after capture.

All this merely means that, in the first place, the stages of death creep over their simple framework so quietly that they are decaying before we know that they are dead; and, in the second, that protoplasmic death is more nearly identical with somatic.

Finally, in the descending scale, we reach unintegrated colonies of cells or single cells. Here there is identity between the protoplasmic individual and the soma; there is no somatic mechanism to break down, no chain along which protoplasmic death can creep from one cell to another.

It is clear, then, that somatic death is an attribute of the metazoa, a merely necessary result of their complexity of organisation, and that somatic death becomes a more and more striking occurrence as the metazoon becomes higher and more complex. It is hardly necessary to point out that a line of argument precisely similar applies to somatic life, and that what are usually meant by life and death are merely somatic life and death, the merest mechanical results of complex mechanism.

It is true that the protozoa are untouched by somatic death, that it has and can have absolutely no place in their life-cycle; that were somatic death the only death in the world, protozoa would be not potentially, but actually, immortal. It is equally true that no metazoan, however lucky, is at all likely to escape somatic death, is at all likely to be immortal. Even in the simplest metazoon, the life of the soma is bound up with the life of the units—or, at least, with the life of groups of the units—and the chances of protoplasmic death of any cell, or group of cells, being followed by somatic death, continually increases with the complexity of the organism, and increases in geometrical ratio as the number of component parts liable to accidental or intrinsic break-down.

The protozoa, then, and chains of single cell-life, like the

unicellular chain of germ cell-life of which the metazoa are the carriers, may be called potentially immortal, and the metazoa mortal. But all this merely deals with somatic death, and is, so to speak, a biological accident. And so discussion or argument dealing with the possibility of immortal protozoa giving rise to mortal metazoa—by, for instance, some unequal fission separating a hypothetically mortal part of the plasma from a hypothetically immortal, is futile.

There is left, then, protoplasmic death. It is certain that protoplasmic death is not a necessary concomitant of protoplasmic life. We know of no means to secure the set of conditions which we call life, without the presence of some small piece of living material; we have considerable reason to suppose that spontaneous generation does not occur, at least as a normal factor in the organic world; we know with absolute certainty that it never occurs in metazoa. From this it follows that a direct continuous chain of cell-life binds all animals with at least unicellular animals; that, for instance, we ourselves are at the end of a simple chain of cell division stretching through an inconceivable number of generations to an inconceivably remote past. So far as experience goes we can certainly say that living matter does not necessarily carry within itself the seeds of death. On the other hand, we can with equal certainty say that death apparently comes sooner or later to all chains of life; though it may be only that in chains of life, as in single lives, old age brings with it increased chances of death. Thus if we review the procession of animal life in the past history of the earth, we find that always they are the topmost branches that wither; that sets of animals highest in the scale at any geological period are constantly dying out and being replaced, not by their own descendants, but by the descendants of a younger and fresher stock. I take it that this does not necessarily imply the otherwise improbable conception that death is a necessary incident in life. Just as the older a weapon or an ornament is, invariably it is the more battered or rusted, because, however jealously guarded, increase of years means increase in number of chances of exposure to accident; so, however nicely adjusted to environment, however constitutionally plastic and ready for adaption to change, lapse of years must almost necessarily bring the one set of unsuitable conditions, the finally fatal moulding of the plastic constitution of an organic stock.

Exposure to accidents and somatic death are sufficient causes of death in the metazoa. It is not necessary to suppose that their cell protoplasm differs from the cell protoplasm of protozoa in having a limited duration of life.

Now let us turn to the two cases of practical immortality to see whether there be any special conditions.

First for the protozoa. It has been too easily assumed that in their case death follows from the gross accidents of life. The protozoa of the Crag, or of the Chalk, or of older beds, are not identical

with living forms. In these beds, as in the ooze of existing seas, the invariable presence of large numbers of uninjured shells seems to point to protoplasmic death as having at least a possible place in protozoa. But existing protozoa as other existing animals are the direct descendants of the protozoa of long ago. They have established a weighty claim to be considered conditionally immortal. They enjoy two special advantages. By living singly they stand or fall by their own luck; they are not tied to the accidents of other cells in a cell-commonwealth. Their easier contact with external media, and readier means of getting rid of waste products, or of accidentally-taken dangerous substances, makes them less subject to the intrinsic accidents associated with cell-life.

Germ cells, again, unlike the working cells of the body, lie comparatively dormant within the tissues until the time comes for the playing of their own part. Practically all the ova a woman produces are already formed in the newborn babe. They lie in the ovary, secluded from the wear and tear of cell-life, subject to few dangers, and as far from the vicissitudes of somatic life as possible. And, lastly, the invariable extrusion of at least one polar body from every ovum is a mysterious and unique condition; a condition which may be a special effort to remove from the cell bye-products the slow accumulation of which in tissue-cells ultimately causes disease or disturbance.

We know that all other cells do ultimately die, and that death of all kinds can be reduced to cell-death. The meaning of cell-death and the meaning of cell-life will be discovered together. But already we know something of the processes going on in living cells. In the first place there is decomposition. We cannot trace all the details, but we know that a number of substances are taken in, so to speak, at one end, and that a number emerge at the other. It is futile to say that the processes are not the processes of chemistry, as we do not know the constitutional formulæ of many of the substances which enter or emerge. But the sum of the activity is a slow feeding of substances of a high potential; a complicated flux of these within the cell; and an emergence of substances of a lower potential. Clearly there are a large number of ways in which such a process can be disarranged. A sudden stimulus may set off the liberation of energy so rapidly that there is none left to do the work of bringing in more food, and starting the chemical reactions. Some waste substance produced accidentally or usually in small quantities, and not regularly excreted, may slowly accumulate until it clogs the whole action. Even in inorganic chemistry, reactions are often apparently so capricious that we know they must be influenced by a multitude of apparently trifling conditions. Lastly, the introduction of some unwonted substance may arrest or supplant the normal reactions.

For the elucidation of all these, there is wanted much experimental organic chemistry. Above all, there is wanted a careful

comparison of the constitution with the effects of the different poisons which act on protoplasm. This is already leading towards a knowledge of the actual occurrences in a living cell, and it will no doubt get more and more clear that the events which cause protoplasmic death are simple interferences with the cell economy arising from internal or external accidents. As in bacteriology, the idea that the fatal influence of these parasitic organisms is due to a struggle between life and life, has been displaced by the idea that it is due to the direct action of a poison accidentally discharged by the parasites, so the causes of protoplasmic death will be attributed less and less to any inherent mortality in protoplasm. There seems no good evidence that protoplasm in suitable conditions, with sufficient food, should ever die, just as there is no reason that a flame, with a constant supply of fuel, and removed from violent draughts, should not burn from the beginning of time to the end of time. But the more complex flame of life is disturbed by many agencies, and these agencies cause protoplasmic death. In the metazoa, where each cell may suffer by the accidents of every cell, a new danger, the danger which is here called somatic death, is so urgent as to be fate rather than accident.

P. CHALMERS MITCHELL.

IV.

The Geology of the Central Himalaya.[1]

THE physical structure of the Himalaya must always be a subject of great interest to geologists, since our knowledge respecting the birth and growth of great mountain-systems is derived very largely from this region; and the imagination is stirred by learning that the main disturbance which brought the rocks to their present elevation took place after the deposition of the London Clay.

For more than fifty years information has been accumulating on the geology of the Himalaya, but no systematic survey had been attempted of the region now described, until Mr. Griesbach commenced work among the higher mountains in 1879.

We can understand how unintelligible the geology appeared at first, the slopes being covered with masses of *débris*, and the peaks being, most of them, inaccessible. Pushing on, however, from one tract to another, the author found many areas where the rocks were exposed, and in course of time the comparisons that could be made, enabled him to piece together the evidence and establish the sequence of rocks.

In the first part of his memoir, Mr. Griesbach deals with the Physical and Stratigraphical Features, and in the second he gives particulars of the sections, and a summary of results. The first part and the summary are naturally the more readable portions of the volume. Attention is drawn to the work in the same or neighbouring regions of other geologists, amongst whom we note R. Strachey, Stoliczka, H. H. Godwin-Austen, McMahon, Middlemiss, and Lydekker.

The highlands of Central Asia comprise the great Tibetan plateau, tablelands traversed by some mountains, and bordered on the north by the Kuen-Luen range, and on the south by the Himalaya. On these tablelands we find the head-waters of a few of the great rivers which intersect the mountain-belt of the Himalaya, and thence find their way to the sea across the plains of India. Thus, the Sutleej rises in the southern part of this plateau, in the region known as Hundés.

[1] *Memoirs of the Geological Survey of India*, vol. xxiii. The Geology of the Central Himalayas, by C. L. Griesbach, C.I.E., Superintendent of the Geological Survey of India. 1891. Pp. i.–x., 232, and i.–xix. (index).

The Central Himalaya is divided into two portions:—(1) the Southern Range, which, including many of the higher mountains, may be said to begin with the mass of the Nampa Peaks on the north-west frontier of Nepal, and to end near the Spiti valley; and (2) the Northern Range, which stretches parallel to the other range, in some places separated by broad valleys 15,000 feet or more above sea level, and in others connected by ridges.

The Southern Range of the Himalaya averages 20,000 feet in elevation, and from 16,000 feet upwards there is perpetual snow. It is a true range though not a continuous ridge, being deeply furrowed by rivers that rise to the north. The rocks are mainly crystalline, being gneiss and schists with great granite intrusions.

The Northern Range is more continuous, but of lesser elevation, and comparatively free from snow. It consists of a system of great flexures of sedimentary rocks, ranging from Palæozoic to Tertiary, and their description forms the chief subject of the present memoir.

Some notion of the physical features of the area may be gathered from the foregoing statement; but it may be interesting to quote the following passage:—

"When travelling northwards from India to Tibet, through the deep valleys and over the great passes of the Himalayas, the eye becomes so accustomed to stupendous mountain masses, and a seemingly interminable succession of ranges and steep cliffs, that when finally reaching the last crest of the watershed, the view over the Hundés Basin which meets the eye, fairly takes one by surprise. At one's feet stretches, as far as the eye can see, an apparently level expanse of country, a level in which, at that distance, all lower ridges and inequalities seem to disappear. All of it is more or less of a brownish-green colour—bare patches of gravelly soil with streaks of bright green pasturage. Not a soul, nor living creature apparently, inhabits this immense waste, only rarely and at great intervals one may detect a solitary black tent belonging to a party of nomadic shepherds, whose flocks may be grazing in the sheltered depressions of this great expanse, where better pasturage is possibly found."

Beyond this great expanse of flat country may be seen the serrated elevations of the Kaílas range, sparingly covered with snow, although the heights average about 18,000 feet.

The structure of the Himalaya is broadly that of a succession of inverted flexures, leaning over towards the south-west. Two great anticlinals, formed of immense flexures of older crystalline formations, can be distinguished. Fossiliferous stratified rocks are preserved in the synclinals, but faulting and the intrusion of igneous rocks have modified the normal order in places; for the inverted flexures are often pushed one over the other along planes of dislocation, that form oblique faults of great magnitude, and produce the "scale-structure" so common in most areas of folding. The Southern Range is formed perhaps of the larger of these main anticlinals, and there is only left what might be taken to be a conformable series of metamorphic strata. North of this great inverted flexure there follows a huge

synclinal trough in which the highly contorted and crushed sequence of sedimentary rocks—great thicknesses of Palæozoic, Mesozoic, and even Tertiary strata have been upraised to stupendous altitudes. Later Tertiary and Recent deposits have been laid down in areas between the great lines of parallel flexure, and these help to form the great tablelands of Tibet and the Gobi.

Most of the great lines of flexure must have existed in Palæozoic times, "yet the great lateral compressions, which pushed up the enormous masses of the Central Asian plateau, with its fringing rims of mountains, were evidently formed after the Miocene beds were deposited, for we see the latter contorted and crushed, under a cover of almost horizontal young Tertiary deposits, which so far have suffered little from the slow action of folding, which it may be assumed is still going on."

The oldest rocks consist of granitic gneiss, regarded as metamorphic granite; they are apparently overlaid by a schistose series, termed the "Vaikrita system" (from a Sanskrit word indicating "metamorphism"). The rocks of this system comprise mica-schists, talc-schists, and thin bands of gneiss; and they are regarded as of sedimentary origin. They seem to pass gradually into a newer series of quartzites, phyllites, and slates, with coarse conglomerates, grouped under the name "Haimanta system."

This system (taking its name from a Sanskrit word signifying "snow-covered") appears to be constant in lithological aspect, and to have a thickness of from 3,000 to 4,000 feet; but the beds are extensively crushed and folded. The rocks underlie conformably Lower Silurian strata, but as they may comprise not only the Cambrian but still older groups, it was thought best to apply a distinct name, even though it be a provisional one.[2] The name itself is meant to indicate the fact that the beds, with few exceptions, lie within the area of perpetual snow. Hence they cannot reveal their history very readily to the geological enquirer. The Haimanta system has yielded only a doubtful Crinoid, a doubtful Bivalve, and an undetermined species of *Bellerophon*.

Conformably above the Haimanta system come beds grouped as Lower and Upper Silurian. Limestone is the predominant rock of the Lower Silurian, and quartzite of the Upper. They follow on without break, passing gradually one into the other by alternating beds. The Lower Silurian rocks are estimated to have a thickness of from 200 to 300 feet only. The fossils, which include Encrinites and Corals, await description, but the fauna of the "Coral-limestone" is said to correspond with that of the Lower Silurian of Europe. The Upper Silurian strata attain a thickness of 1,000 to 1,200 feet, but their fossils, which include *Orthis* and other Brachio-

[2] It should be mentioned that somewhat different names were previously adopted by Mr. R. Lydekker for the great groups of rocks in the adjoining area of Kashmir, &c. *See Mem. Geol. Surv. India*, vol. xxii., 1883.

pods, as well as Corals and Encrinites, also await determination. We must therefore suspend judgment concerning the European equivalents of these strata, and the same remarks apply to succeeding formations of Coral-limestone, Crinoid-limestone, white quartzite, and limestone, that are said to rest conformably on the older strata, and to represent formations from the Lower Devonian to the Upper Carboniferous. The limestones are variable in thickness, and the entire series varies from 1,450 to 2,500 feet. In the rocks grouped as Devonian there are *Orthoceras* and *Productus semireticulatus*.

The occurrence of *Athyris royssii* and of *Productus* in the highest band of limestone affords the only particular evidence stated of its Carboniferous age. The distribution of the highest Carboniferous beds affords evidence in overlaps of physical changes of a widespread nature, and the author remarks that perhaps no other formation in Asia has a wider area of distribution. He lays perhaps too much stress on the lithological change between the beds grouped as Carboniferous and those regarded as Permian: even abrupt changes from quartzites associated with limestone to "crumbling black shales" afford no criterion of *great* physical changes as proved by the record of our British Carboniferous rocks. The fact, too, that the succeeding beds in the Himalaya consist of *Productus*-beds of Permian age (yielding a form near to *Productus latirostratus*) tends to show, as the author admits, that the break could have been of no great extent as regards time. The evidence of a break lies chiefly in the uptilting and partial erosion of the Upper Carboniferous beds, so that successive strata are overlapped in an unconformable manner by Permian rocks. Thus the Carboniferous beds, which are purely marine, are followed by a great system of strata that are in a large measure littoral; and the author considers that the wrinkling process, which resulted in the elevation of the Himalaya, was partially brought about in Permian times, so that the main outlines of the great range, which had previously been determined, were then more distinctly foreshadowed.

The older rocks are regarded as forming a continuous series; and now, after the unconformity just mentioned, we find the records of another sequence from the Permian shales to Liassic limestones. The Permian beds exhibit a thickness only of 120 to 250 feet, but they have suffered much from crushing and folding, and the more rigid Triassic rocks have been thrust over them on to the Carboniferous formations. Passage-beds occur between the Permian and Trias containing a fauna akin to both, and noted as the *Otoceras* horizon, or zone of *O. woodwardi*. The Triassic rocks attain a thickness of from 1,500 to 4,000 feet, and consist mainly of limestones and shales. They are divided into Bunter, Muschelkalk, and Keuper, and are characterised by fossil zones, of which Brachiopods are the chief indices. Casts of *Corbis* occur in the Upper Trias, and in some of the beds *Ceratites* and *Arcestes* are preserved. The author remarks that a great likeness exists between the Trias and Rhætic

beds of the Himalaya and the Alpine regions. As there is a complete passage upwards from the Permian, there are probably represented some strata not found in localities where there is evidence of a break between Permian and Bunter.

The Rhætic beds comprise a great thickness of dolomites and limestones, characterised by *Lithodendron* and *Megalodon* in the lower beds, and with Crinoid-limestones and Brachiopods of Upper Kössen facies in the higher portion. Strata regarded as Passage-beds between the Rhætic formation and the Lias, yield *Pecten valoniensis*, *Cardium rhæticum*, some Liassic *Limas*, and other fossils. The thickness of the Rhætic formation is upwards of 2,000 feet; and the beds are surmounted by black shales and dark earthy limestones, with oolitic structure altogether about 100 feet thick. These beds are assigned to the Lias; and they contain *Ammonites annulatus*, *Belemnites*, *Gervillia*, &c.

Some confusion is caused by the employment of the term Jurassic in the sense in which Oolitic series is used in this country, so as to exclude the Lias; a grouping that is not according to general custom.

The beds that succeed the Lias are the Spiti shales, so named by Stoliczka; they attain a thickness of 1,300 feet, and embrace the entire series of deposits between the Liassic strata and the grey and greenish sandstones that are assigned to the Lower Cretaceous system. From these Spiti beds, which are, perhaps, the best known and most widely-distributed of the Himalayan formations, numerous fossils have been collected, not only by geologists and travellers, but by natives of Tibet and India. The author tells us that from ancient times a trade in the Ammonites has existed; great quantities of them being brought every year to India, chiefly to the holy places of Hindu pilgrimage, to be sold as relics to the worshippers.

Although no unconformity can be traced in the stratification of the Lias and the Spiti shales, there is an alteration in lithological character, accompanied by a sudden change in fauna. There is, apparently, an absence of the Lower Oolitic fauna (Lower Jurassic of the author), but the lowest portion of the Spiti formation has yielded no fossils. Higher up there are found Ammonites of Oxfordian type; and the upper Spiti shales are of Tithonian age.

The Spiti shales are much affected by the grand flexures; they have frequently been squeezed into the narrowest folds, while the rigid Cretaceous strata have been pushed over them.

The Spiti beds pass gradually into strata that are grouped as Lower Cretaceous or Neocomian, and the same succession is found to occur through the northern part of Persia and along the shores of the Caspian. These "Lower Cretaceous" beds, 1,200 to 1,500 feet thick, consist mostly of sandstones and shales. They yield a few crushed *Belemnites*, and it is just possible they may be of Upper Tithonian rather than of Lower Cretaceous age. They are overlaid by

white or grey limestone, containing *Rudistes*, *Inoceramus*, and other marine Upper Cretaceous fossils. These upper beds are widespread, and although in the Himalaya and immediately adjoining areas there is no visible unconformity with the beds below, yet in areas beyond there is evidence of decided overlap accompanied by unconformity. Regarding the Tertiary strata, it is found that there is a marine Nummulitic formation, apparently conformable to the Upper Cretaceous, much disturbed and altered by masses of igneous rock. This formation is overlaid unconformably by sandstone, probably of Miocene age, and resembling some lower Siwalik Beds, and above these rest irregularly the younger Tertiary strata of Hundés. These younger Tertiary strata form a striking feature in the geology, as they are found at elevations of 14,000 to 16,000 feet, and preserve an almost horizontal character. They are of great thickness, and cover all the older Tertiary strata, and perhaps the Secondary rocks which form the trough of Hundés. Mr. R. Lydekker has shown that these beds are probably younger than the Siwaliks, almost certainly not older than Upper Pliocene, and very likely of Pleistocene age. The position of the strata and their structural relations forbid the notion that any great change in physical conditions has taken place since their deposition. They appear to be of lacustrine formation.

The work is admirably illustrated. There are two large geological maps, six plates of sections, seven panoramic views and profiles, taken with the aid of the camera lucida, and sixteen photographic pictures reproduced by the process of heliogravure. These pictures are exceedingly well executed, and they show various forms of mountain scenery, glaciers, rock-structure, and contortions.

H. B. Woodward.

[Since the above work was published, we learn that Mr. Griesbach forwarded to Vienna many fossils of the Mesozoic age, which he had collected during his geological explorations in the Central Himalaya. These fossils were examined by a specialist in Alpine palæontology, and found to correspond in a remarkable way with those of the alpine strata. Such a fact has so interested the authorities of the Royal Imperial Academy of Science, Vienna, that they have, with the co-operation of the Government of India, determined to send out an exploring party to the Central Himalaya to compare their geological features with those of the Eastern Alps. Dr. Carl Diener, Lecturer in Geology at the University of Vienna, has been chosen leader of the party.]

V.

The "Albatross" Expedition.[1]

THE brief sketch of the work accomplished by the U.S. Fish Commission steamer "Albatross" last spring in the Pacific Ocean, now given to us, is most interesting, and stimulates the expectation with which we look for more detailed results in the near future. The cruise lasted from February 22 till April 23, and may be said to have consisted of four distinct stages (1) from Panama to Cocos Island and Malpelo Island and back to Panama, (2) from Panama to Galera Point and the Galapagos Islands, (3) from the Galapagos Islands to Acapulco, (4) from Acapulco to Guaymas in the Gulf of California. The work accomplished comprised dredging and townetting, both on the surface and at various depths, temperature and current observations, and some investigations into the physical and biological conditions of the Galapagos Islands. We shall endeavour to summarise as briefly as possible the results of these various lines of research.

The contour of the sea-bed on the Pacific side of the Isthmus of Panama is very different from that observed on the Atlantic shore. On the latter we have great submarine banks extending from Yucatan, Honduras, and Venezuela, with deep basins in the Gulf of Mexico and the Caribbean Sea. On the western side there is a deep trench more than 2,000 fathoms deep, widening as it approaches the land, and maintaining this depth till it is not far away (often within 100 miles) from the shore. The Galapagos Islands form the summit of a large bank, shaped like an inverted comma, the 1,500-fathom line stretching out in a point which takes in Cocos Island, and almost meets the line of similar depth from the shore just off Cape Mariato. Comparatively few soundings have been made among the Galapagos Islands themselves, and no detailed contours are given on the chart of this group.

The bottom deposit was most often characteristic Globigerina ooze, and on one occasion the trawl was completely filled with a species of *Rhabdammina*. At two places near Cape Mariato patches of modern greensand were discovered. It is important to notice that

[1] General Sketch of the Expedition of the "Albatross," from February to May, 1891. By Alexander Agassiz. *Bull. Mus. Comp. Zool.*, xxiii., pp. 1–89, 22 maps and plates, 1892.

even at the greatest depths, and at the greatest distance reached from the shore, distinct traces of terrigenous material were found in the deposits. Sometimes this was a fine sticky mud, at other times there were large quantities of decayed vegetable matter. In fact, during certain parts of the cruise "there was hardly a haul taken which did not supply a large quantity of water-logged wood, and more or less fresh twigs, leaves, seeds, and fruits in all possible stages of decomposition."

The temperature observations are given in considerable detail, and as the region investigated is traversed by some half-dozen different currents, it would be impossible to give an adequate summary of the results without the aid of maps and diagrams. The most interesting point to notice is that no sufficient evidence was obtained of the updraught of cold water along the western coast of equatorial America, to which Dana and others have attributed the absence of coral reefs in those regions. Agassiz rather believes this to be due to the amount of silt brought down from the land.

The next section of the Report is the longest, and one of the most interesting. It is devoted to a discussion of the bathymetrical distribution of pelagic life. Professor Agassiz first reviews the older inquiries in this direction, and shows that with scarcely an exception they are all vitiated by defects in the apparatus, and that they are quite inadequate to demonstrate the existence of a populous fauna at intermediate depths, as stated by Professor Haeckel in his "Plankton Studien." In fact, he enters upon quite a sharp passage of arms with the philosopher of Jena, so sharp, indeed, that it must be read to be appreciated.

Passing by this controversy to consider the facts obtained by the "Albatross," we may remark that the cruise will be memorable for the use which was made of a really practical opening and closing net, "which worked to perfection at 200, 300, 400, and 1,000 fathoms, and had the great advantage of bringing up anything it might find on its way up above the level at which it was towed." This apparatus was due to the ingenuity of Captain Tanner. The net is conical in form, and is suspended from a ring between two ropes, which are weighted at the bottom. About one-fourth up the net two strings encircle it, passing through loops; to the ends of these strings are attached weights which, when allowed to fall, draw them tight, and constrict off the lower portion of the net. They are, however, held up by a crank, and can be liberated when required by a sliding weight or messenger, which is allowed to run down the line. The difficulty of opening the net under water is obviated by the simple expedient of letting it down with the pointed hinder end first, the pressure of the water being sufficient to keep it pressed tightly together like a folded umbrella. Arrived at the desired depth, the vessel is put in motion, and rope enough is paid out to keep the net moving horizontally, this being judged of by the slant of the rope.

After towing for, say, twenty minutes, the net is again brought to the vertical, and the messenger sent down to close it; after which, it is brought up to the surface, and the contents of the upper and lower portions of the net examined separately.

This net was used on several occasions in water of considerable depth, and undoubtedly failed to produce evidence of a mid-water fauna of any variety or abundance. The number of unexceptionable trials at depths of 400 fathoms and upwards was, however, too small to justify any far-reaching conclusions.

The Galapagos Islands occupy the next section of the Report. This group consists of some nine or ten moderate-sized islands with several small ones. With the exception of a small colony maintained on Chatham Island, they are, as when first discovered, uninhabited. Their vegetation presents three distinct belts or zones. The first, in close proximity to the shore, is characterised by stunted salt-loving plants; among them are a Verbena and an Acacia with small leaves and inconspicuous flowers, and a large tree, the Palo Santo. Where the lava prevents the growth of anything else, we still have an *Opuntia* and a *Cereus*. As we ascend the latter disappears, as also the Verbena, while the other two increase in size. The second belt is found where the lava has become decomposed into soft reddish earth, on which grow woods of small trees resembling the Andean *Polylepis*. From this there is a gradual passage to the third region, which is bare of trees and covered with coarse grass. The bulk of the vegetation seems to have been derived, as might be expected, from Central America. The amount of decaying vegetable matter brought up by the trawl has already been alluded to, and, as Mr. Agassiz remarks, "certainly offers a very practical object-lesson regarding the manner in which" these islands must have received their vegetable products. Autotype reproductions of a number of photographs taken on the expedition show very clearly the general features of the vegetation.

The land fauna does not seem to be abundant: the giant tortoises, once so numerous that single vessels have been known to carry off seven hundred at once, have now disappeared. The only tortoise obtained by the "Albatross" was got on Duncan Island: it was sent alive to Washington. A bat is known, but appears to have been on these islands only for the last six or seven years. The insects are not numerous; caterpillars were very abundant, but only of few species. Quite a remarkable number of domestic animals have now become feral.

A great deal of coral rock, in isolated patches, is found about these islands, and the so-called "sandstone" proved, on examination, to be a slightly oolitic coral rock. There are, however, no true reefs either on these islands or anywhere in the Panamaic region.

The author discusses the recently expressed views of Dr. Baur on the origin of the Galapagos Islands, and comes to the conclusion that

they are distinctively and entirely volcanic, and have never been connected with the mainland of South America, from which they are now separated by a wide plain nearly 2,000 fathoms in depth.

The observations upon the deep-sea fauna are most important and interesting. The "Challenger" Expedition traversed the centre of the Pacific Ocean, along a line extending through the Sandwich Islands to Tahiti. The "Albatross" examined a wedge-shaped portion of the wide expanse lying to the eastward of this line. One of the most striking facts elicited was that there exists in this area a "considerable fauna, which finds its parallel in the West Indies, and recalls later Cretaceous times, when the Caribbean Sea was practically a bay of the Pacific."

Some differences may be noted—in the Pacific area the scarcity of Ophiurids contrasts strongly with their abundance in the Atlantic, "where it often seems as if ophiurans had been the first and only objects created."

The general absence of Crinoids and *Comatulæ* was noticeable, although one very interesting new genus (*Calamocrinus*) was obtained from 782 fathoms off Mariato Point, an elaborately illustrated description of which has already been published.[2]

Among Bryozoa further examples of the interesting genus *Kinetoskias*, first obtained by Danielssen off the Norwegian coast, and subsequently by the "Challenger" at several localities in the Atlantic Ocean, have been dredged.

Several Acalephs (*Atolla*, *Periphylla*), certain *Beryx*-like fishes, Scopelids, Stomias, Melamphäes, must now be excluded from the abysmal fauna in which they have hitherto been reckoned, as they have been captured by the Tanner net in depths less than 300 fathoms.

A brief comparison is presented to us of the relations existing between the Echinoid fauna of the two sides of the Isthmus of Panama, with a promise of a more detailed statement hereafter. Only one genus of urchins (*Salenia*) has been discovered in the deep waters of the Pacific side which has not previously been found on the Atlantic side. The interesting Echinothurids appeared, but these, as is well known, have an extensive Pacific as well as Atlantic distribution.

An important feature of the work on board the "Albatross" was the number of coloured sketches prepared by the artist, Mr. Westergren. Some of these have reached this country, and will be most useful in supplementing descriptions of specimens preserved in spirit.

A considerable amount of information is given in the present Report regarding the colours of various deep-sea types; these are very variable, but there seems to be a preponderance of violet. In many cases there seems to be a great lack of adaptation to the

[1] See NATURAL SCIENCE, No. 2, p. 93.

greenish-grey mud in which the animals live. Other creatures, however, furnish just as conspicuous examples of mimetic resemblance, witness a small crab, whose back resembles a bit of muddy area covered by corals, while its huge white arm suggests a fragment of an Isis-like Gorgonian.

The detailed reports upon the various groups of animals will be written by different specialists, and the whole work when complete will furnish a most welcome addition to our knowledge of the Pacific Ocean. The "Challenger" gave a rough sketch of the conditions of deep-sea life; such expeditions as that of the "Albatross" help to fill in the details of the picture.

W. E. HOYLE.

VI.

Professor Lloyd Morgan on the "Grammar of Science."

PROFESSOR PEARSON'S "Grammar of Science" occupies an almost unique position among English books which treat of the nature, scope, and limitations of Scientific Method. Whereas most other writers have treated the subject descriptively and historically, as did Whewell, Herschel, and Baden-Powell, or have busied themselves with the logical processes employed, as did Jevons, Professor Pearson has tried to bring about a more accurate use of scientific terminology by a criticism of the fundamental concepts of modern science. In this criticism he has endeavoured to co-ordinate the concepts of the sciences with those of the theory of knowledge.

Professor Lloyd Morgan is fully alive to this feature of Professor Pearson's work; but to a careful reader of the latter it must appear that in many respects the reviewer has either misunderstood the doctrines which he criticises, or has failed to establish a consistent ground for his rejection of them. Professor Pearson adopts a philosophical position which may be fairly described as thoroughly idealistic. In this respect, indeed, he is at one with most writers of text-books on Physics. Only with a difference. The text-book writers are generally as idealistic as Hume or Mill in their opening paragraphs, but they fail to show their faith by their works in the chapters which follow. "The Grammar of Science," however, gives in a clear but concise manner the author's own grounds for the position he has taken up, and, what is even more to the point, the actual bearing of this theory of human experience upon the thought and language of science is illustrated by abundant example as well as by precept.

Thus, even if the book had no other value, it would at least be of service to abate the assurance of those writers who suppose that though Idealism sounds well in the lecture room, it is nevertheless doomed to inevitable scientific bankruptcy in the fields of practical research.

Now the odd thing about Professor Lloyd Morgan's position is that, although he is himself, as he tells us, fully impressed with the general truth and value of these idealistic results in their proper place, he yet seems to think that their proper place is on the shelf.

In truth, Professor Lloyd Morgan is too good a psychologist wholly to reject them; but he has not quite the courage of his convictions, and he hesitates to trust his full weight upon a bridge the soundness of which he professes not to doubt, but which still, to his mind, seems too shadowy to be solid. He fears that confusion will result. Professor Pearson shows that the danger of obtruding idealistic results, in themselves true and valuable, into the conclusions of the several sciences is an imaginary danger, but that harm has already come about through the neglect of them.

We may grant that the analysis of the nature of experience is on one plane of investigation, and molecular physics on another, but by shutting our eyes to the relation between them we (even now) run some risk of framing a theory of Physics the language of which may prove to be incompatible with our theory of Thought.

In his discussion of the various problems raised by Professor Pearson's book, Professor Lloyd Morgan begins with that of the "external object" and the nature of "reality."

Here, at any rate, by his own confession in "Animal Life and Intelligence," it would seem that the reviewer is in substantial agreement with his author; but he is strangely diffident. Often he seems to say, "I know that this is all true, but let us make believe that it is not."

Concerning "object and eject," Professor Lloyd Morgan, by a strange oversight, has somewhat missed the point in dispute. The question is not, as he supposes, as to whether or no some physical link is necessary to enable one mind to appreciate the thoughts of another. It is whether or no, by any imaginable means, the thoughts of A. could become something more than matter of inference, and in short be directly an object in the consciousness of B.

Passing over the chapter on "Scientific Law" (perhaps the most practically important in the book), and those on Cause and Effect, and on Probability, Professor Lloyd Morgan deals next with the subject of Space and Time. There can be no doubt that the doctrine as to the nature of space and time which is associated with the name of Kant is the most important positive addition which has been made to the idealistic theory of human knowledge since the days of Bishop Berkeley; and it is one of the noteworthy features of the "Grammar of Science" that here, perhaps for the first time in the writings of English men of science, we find at once a full recognition of the general truth of Kant's doctrine, a short but clear exposition of it, and, in subsequent chapters, an explicit application of it to the problems of Matter and Motion.

Now, on this view of Space and Time there is much to be said. Supposing it to hold its ground as a truth of philosophy, and to acquire the place which it deserves in any complete account of human psychology, there is still room for debate as to the best mode of stating it, both for general purposes, and especially for the more

technical purposes of actual physical science. Here, if anywhere, comment and criticism were to be hoped for. But our hopes are doomed to disappointment. All we get from Professor Lloyd Morgan is the following:—

"The question is whether, given certain sense impressions which suggest the percept clock, there are any objective relationships which I can describe by using the adjectives distant, extended, and successive."

Now it is almost incredible that one who has carefully read Professor Pearson's earlier chapters, and who may also fairly be supposed to be not wholly unacquainted with the writings of Kant, should really for a moment suppose that *this* is the question. The real question, of course, is not whether there are any relations of objects which may be so described, but what it is in truth which we are stating when we make use of these terms of relationship. The adjective "objective" (which Professor Pearson has happily avoided in his chapter on "Space and Time"), it is almost needless to say, settles nothing. But to Professor Lloyd Morgan the blessed words "objective," "frankly objective," and "objective existence" (without a hint of further definition), seem to be conclusive shibboleths as to the great problems upon which Berkeley, Hume, and Kant spent the best working hours of their lives.

What does Professor Lloyd Morgan mean by "objective relationships"? Does he mean that they have an existence, as such, in the absence of the percipient mind? This he expressly denies. Does he mean a relationship between objects? No one has denied objective relationships in this sense, and Professor Pearson on his part declares, "Space has no existence if objects are withdrawn."

It would seem that what Professor Lloyd Morgan means by "objectivity" is "empirical reality." In other words, just as Dr. Johnson supposed that Berkeley's views on Matter must lead men to run their heads against brick walls, so Professor Lloyd Morgan has forgotten that human experience is none the less real, either as to weight or measure, even if it can be shown that the terms which express that experience owe all their significance to their inseparable association with the nature of our perceptive faculty.

Physics, no doubt, is wise, as Professor Lloyd Morgan remarks, in dealing with space and time in frankly objective terms. This Professor Pearson has not disputed, at least in any intelligible sense of that phrase. What he has disputed is the conventional, or uncritical view, which takes space and time for "things," instead of taking them for an order of things, and which supposes that bodies are capable of being philosophically dealt with as if they had a "reality" not conditioned by the nature of our faculties for knowing them.

Moreover, whether true or not, all this is to Professor Lloyd

Morgan out of place in Physics and Biology. And yet the question *has* practical bearings as will be evident to any reader who carefully studies the chapter on Probability, or the concluding section of the chapter on Space and Time in the "Grammar."

Happily, there can be no two opinions about the importance of clearly distinguishing between Perceptions and Conceptions, and by the aid of an analogy Professor Lloyd Morgan very clearly illustrates the process by which, in many cases, scientific conceptions are reached. But for whose benefit has Professor Lloyd Morgan written the sentence which follows, in which we are told that "the perceptual conception (*sic*) as such has neither a continuous boundary as idealised in geometry, nor a molecular constitution as idealised in physics"? Professor Lloyd Morgan must have forgotten a passage in Mill's "Logic," which tells the reader that "although I cannot dig the ground unless I have the idea of the ground, still it would be ridiculous to describe digging as putting one idea into another."

The Geometry of Motion is left to the Physicists to criticise.[1] This chapter, together with those on Matter and the Laws of Motion, contain a full exposition of what the author means by mechanism in Nature, but the reviewer can say nothing more than that it appears to him more simple and more scientifically accurate to regard matter and motion as frankly objective and ("perceptually") external—a dictum which does not call for further comment.

Some remarks in Professor Pearson's chapter on Life have given Professor Lloyd Morgan an opportunity to defend his well-known views upon the origin of consciousness. The reviewer holds that consciousness has been developed from something which he calls Metakinesis, which something is said to be of the same nature as consciousness, but to have not yet risen to the level of consciousness.

Professor Pearson thinks that we might as well talk of life as having been developed from something "which-is-not-yet-life-but-which-may-develop-into-life."

There seems but small chance that the author of the "Grammar of Science" will be led to believe in Metakinesis by Professor Lloyd Morgan's new defence of it, for, judged by the principles of that work, the scientific legitimacy of the theory is more than doubtful.

It is one of the canons of Professor Pearson's scientific method that a Conception, in order to be valid, must be derivable from Perception; also, it is essential to the validity of the Conception that it should be self-consistent. But what phase of our perceptual experience is described by Metakinesis? and what can we say of its self-consistency when we are told in one place that it is phenomenal, and in another that it is arrived at by carrying to a conceptual limit that which is suggested in the sphere of the perceptual?

[1] *See* review in *Nature*, June 2, 1892, and correspondence in *Nature*, June 30, July 7, July 14.

Professor Lloyd Morgan has himself done much admirable work in a difficult field of descriptive science; but it may be doubted whether his criticism of the "Grammar of Science" will (at any rate to those who have read the book with attention) enhance the well-deserved reputation which the first eleven chapters of "Animal Life and Intelligence" have won for him.

He seems to be serving two masters. If certain results *are* true in psychology, we cannot afford to despise them when we come to deal with Physics and Biology. Still less can we neglect them if we must needs discuss the origin of consciousness.

R. J. RYLE.

VII.

Technical Education in Surrey.

IT requires no genius to see that education is as apparently endless as the civilisation with which it is indissolubly connected, and progress in the direction in which the most important nations of the world are developing is dependent very largely on the introduction of mind into labour—the more complete guidance of the hand by a cultivated brain. In England we have been, in some degree, slow to recognise the value of scientific training, and our Continental neighbours are, at the present moment, ahead of us, in the fact that they possess a combination of educated employer and employé. Prosperity in the industries of this country is due primarily to scientific knowledge, and for complete success in its application it is necessary that the efficiency of the worker should not be reduced by want of intelligence.

The Government Grant to the County Councils has rendered it possible for something to be done, not only by way of raising the general level of intelligence, and so indirectly altering the interests of life among the masses, but at the same time in imparting information which will have a practical bearing on definite occupations.

During the last year, nearly every County Council in England has been intensely active in carrying out its Scheme of Technical Education, and the work, as a whole, is undoubtedly a step forward which augurs well for the future. The want of precedent as a guide is sufficient to justify the expectation of failure in some cases, and the gigantic scale of the work, together with its diversity, makes it of great difficulty, even so far as its mere organisation is concerned.

At the outset, the difficulties to be met with seemed almost insuperable, but owing to the energy of the organising secretaries, and the interest manifested locally, classes were held, lectures delivered, and work done in many branches of science at hundreds of towns and villages during the winter months. The great aim of the work has been to combine practice with the science underlying it, and the meaning of Technical Education in its legal aspect hints that the latter should have due prominence.

The most interesting, and at the same time most difficult part of the work is that which is concerned with rural districts. It is in these places that the greatest amount of ignorance prevails, coupled

with a natural apathy for mental exercise, but in power of perception, and even enthusiasm for scientific teaching, our experience with the Surrey labourer leads us to conclude that the stupid countryman is to some extent a myth.

We are far from a universally enlightened state among the rural population, but it only needs a continuous application of well-organised teaching of a carefully-selected kind to make Technical Education among village communities a real power in the country, which will not only lead to a rise in the tone of society, but to an improved physical condition of the people.

The causes of failure, which has been small, are not far to seek. Want of adequate advertisement and explanation of the design is responsible for a good deal. At the commencement last September few districts had any conception of the nature or object of the work, and various schemes were put in force without suitable or sufficient warning. With a people who are inclined to move slowly and consider matters, nothing is more injurious to success than a sudden call to arms—chaos results, and a bad beginning, instead of leading to a good end, leads to total extinction.

Fortunately little injury of this kind has happened, but traces were bound to occur here and there. One of the first requirements in the accomplishment of satisfactory progress in these country districts is the formation of a representative local committee which will endeavour to rouse interest in the general scheme. Too frequently has the work devolved upon single individuals whose efforts, though in many cases excellent, are not likely to have the character of permanency.

It was necessary at the commencement to make some already-existing committee a basis for operations. These, however, were merely temporary, and in over 120 towns and villages in Surrey properly constituted committees exist, and work has been carried out.

The peripatetic system has been resorted to, and a considerable variety of subjects have been taught—health lectures, nursing, dairy work, drawing and woodwork, together with different branches of science—the average attendance at each of the science classes and lectures being between thirty and forty. The writer has been chiefly occupied with the teaching of Botany for agri- and horticulturists, and it is with the experience gained and the conclusions arrived at that this article is concerned.

The practical value of the science which deals with plants is obvious, when we consider that the animal creation depends for its existence on the vegetable kingdom, and no system of education for the population in rural districts can be considered complete without giving a large share of attention to scientific teaching in regard to the life processes and structure of plants.

No science, however, in Great Britain is in a more purely

academic position than the science of Botany. Perhaps this is true to a considerable extent in other countries as well, but the bridge between the science as taught and its practical application, is of such a nature that we may almost say it is non-existent at home. In Germany, France, and even Norway and Sweden, "practice with science"—the motto of our Royal Agricultural Society—is more of a reality than a name. No better evidence of the danger of continuous specialisation—which is very often heedless of the claims of the practical man—is seen than that which confronts us when we compare English scientific botanical literature with the books on which a gardener or farmer has to rely for his information. In the former, pure science for its own sake—not to be heedlessly condemned—exists in a state of supreme ignorance of its co-partner on the side of practice. The illustrations of structure and application of physiological principle in the modern text-book, savour of curiosity merely, or are worn threadbare by being compelled to do duty on all possible occasions. The botanist has gone on and on into minute and endless paths, far ahead of ordinary life, and is lost to the cultivator of field or garden. The gardener, on the other hand, is primarily concerned with the practice of his art, and has, by force of circumstances, specialised in his own peculiar manner, and become almost a mere machine. He has found it difficult to account for success or failure, and has been compelled to register methods minutely, and the result is he is lost to the botanist in blind and often meaningless adherence to custom. Scientific explanation or statement of principle in his books is missing, or, if given, is often worse than if it had not been present at all. The scientific man and the practical man in botany, so far as England is concerned, have left each other almost completely; whatever the one possesses belonging to the other is ancient and valueless. An exchange of books, under such circumstances, can easily be imagined. The gardener, in beginning to study botany, finds himself in a foreign land, and the botanist is not much better off when he attempts to improve his acquaintance with horticulture.

Up to the present time, in this country few attempts have been made to remedy this state of things, but abroad the step between the two is firmly laid, and new discoveries are adapted to everyday requirements several years before we avail ourselves of them.

It is essential for the proper development of scientific work in its best form that it should be kept apart from practical considerations to some extent, but isolation may take place between the advanced specialist and what should be his colleague, and the gap between them becomes so great that they refuse to recognise each other. This is the condition of affairs between Horticulture and Agriculture, and one of their chief corner-stones—the science of Botany.

It is time we made an attempt at a remedy. Up to a certain point the requirements of the farmer and gardener, so far as this subject

is concerned, are alike, and it has been our lot to lecture on "Plant Life" with a view to explain the nature and organisation of a plant, so that reckless treatment of what is too slightly recognised as a living structure should be avoided.

The nature of a seed, its germination—the structure and use of root, stems and leaves, and their treatment—the food materials and their method of introduction into the plant—movement of "sap," water, and reserve materials, all these points and others were taken and scientifically explained in a manner suited to the audience without sacrificing accuracy. At some of the most successful centres the leading employers of labour, the parson, small tenant farmers, gardeners, and labourers made up the gathering. It is a mistake too commonly, perhaps unconsciously, committed, to estimate the success or failure by the number of mere labourers present, but although an important item, there are others whose rank and elevation are higher in whom, perhaps, the first fruits of this scheme will show itself.

In connection with the weekly lectures, questions were given and answers sent in from practically every centre. At one village five or six gardeners sent in papers for the whole course, and passed the examination at the end with credit. It is, indeed, an achievement to have succeeded in obtaining weekly papers from men to whom writing and expressing themselves on paper is a new and troublesome self-denying operation.

Sequence in subjects is aimed at, and the teaching is made more complete and lasting by keeping a correct balance between theory and practice. Lectures on "Plant Life" have been, or will be, supplemented by courses of horticulture by gardening experts. In this department most successful work has been accomplished, and it is only by this combination that progress will be made. It has been stated that horticulture cannot be taught by means of lectures, but it is very doubtful if the science connected with an art or industry can be taught in any other way. The practical results from such teaching last winter are already becoming manifest by the increased yield in small gardens we have seen. Only a few days ago a schoolmaster showed us the effect on his currant bushes which some simple lessons on pruning had had; and after simple scientific lectures on the structure and mechanism of roots or leaves, almost invariably some of the audience have made accurate deductions from the principles involved, and have explained failures in some of their previous experience. It has often been encouraging to learn of the existence of this quiet thought, and have evidence of the living interest taken in the work.

Labourers in possession of a cottage garden are those who are open to the reception of teaching, but for the country gardener, who is often most ignorant of the things he deals with daily, little attraction seems to be possessed by this effort to improve his position.

Few men like to be told of their faults, but there are, indeed, many excellent exceptions to be met with among the younger men in the gardening class. It is astonishing how little real interest in his work is manifested by the average so-called gardener, but we have had considerable experience among them of ability and earnest desire to benefit by the opportunities placed before them. This, perhaps, is especially so where their employers are not wholly ignorant of the principles underlying sound cultivation.

The most promising work is seen when the gentry of the district are interested in the movement, and estimable examples of self-sacrifice and genuine regard for the education of the labouring classes have been met with in several parts of Surrey. A clergyman, in one instance, went to the trouble and expense of hiring a "Technical Education Van," to convey ten or fifteen people to the lectures from a neighbouring village during the whole winter. Aid in many ways has been given for carrying out the scheme, and supplementary teaching has been voluntarily undertaken.

Taken as a whole, the centres whose appreciation stands highest, and where the interest has been well maintained throughout, are comparatively small villages. There the organisation is more easily managed, and there is less likelihood of complication arising from attempts to do too much.

The least successful places are those which are neither towns nor villages, but hybrids between the two. Want of unanimity of sentiment is prevalent among the population, and discontent sometimes arising into a species of party feeling is a block to progress.

In such districts failures are met with which must be attributed, however, to attempts at too diversified a programme. One of the greatest dangers to be guarded against is that which arises from burdening a district with excessive educational opportunities. It is impossible for anyone engaged in manual labour all day to keep up healthy mental exercise after his daily work, and yet many attempt this, only to become disgusted with the scheme in the end. In one of our districts, where results were distinctly worst, *bonâ fide* students were found suffering from want of leisure. Every evening from Monday to Saturday was occupied in some kind of improvement, and time to digest the mental pabulum was missing.

The work which is being inaugurated throughout the country, and which we hope is in a fair way towards completion so far as its preliminary arrangements are concerned, is destined, if properly carried out, to bring about a material and social advance among the masses. No class is more in need of this than the rural population, and we can hardly help thinking that the easiest way of improving the worker in the fields, both in regard to his efficiency and his influence as a factor of society, is by giving him increased interest in his occupation by education, such as is being attempted by the County Council. Nothing seems to be more certain than that the possession of

a cottage garden by all labourers, of such dimensions that it is worth calling by this name, would be a boon to the village community generally in agricultural districts. Those who are intimately acquainted with the country are aware that the material happiness and comforts of the labourer are very often bound up with the possession or want of, not three acres and a cow, but a garden. Time spent in the latter nearly always means time lost to the public-house, and this, together with the material benefits derived from the culture of fruit and vegetables, and the pride evinced in the success of his energies, is sufficient to justify a considerable expenditure and sacrifice of time and money on his education. In this simple matter there is undoubtedly a key to the solution of some of the social problems which are at the present moment beginning to trouble the minds of all thinking people.

It is necessary to consider both the adult population and the rising generation, to some extent, apart from each other. With the former, so long as it lasts, we must be content to make the teaching immediately of service, and suited to their mental capacity; with the latter we must deal somewhat differently, and endeavour to lay the foundation of a more extended education than their progenitors have had.

At one centre practical work in Horticulture has been attempted, with very good results. The local committee have had a piece of land placed at their disposal, which after being fenced in and divided into plots has been handed over for cultivation to the lads of the village who have left school. The work is superintended and explained by professional gardeners, and advice and instruction on the work in hand is given at intervals in the form of an address by one of the County Council's lecturers. Seeds, tools, &c., are provided by the Council, and the produce becomes the property of the pupil. Arrangements are being made to extend the scheme in other parts of the county.

A great deal of power is lost if education is allowed to slip into the background when a boy leaves school for good at the age of 12 or 14. In the country, where there is little incentive or opportunity for improvement, at 20 he is frequently a blank. This is to be kept in view, and in Surrey, schemes for continuation schools are already in active preparation. For this work competent teachers will be required, and a large number of County Councils have set aside and already expended much on lectures and classes for elementary teachers, in order to fit them for extended usefulness. Zeal and intelligence have been displayed by all these students, and it has been possible to make rapid progress. Classes in horticulture, botany, chemistry, and physiology have been carried on during three sessions, and forty scholarships of £10 each have been competed for, which will enable the successful candidates to avail themselves of the opportunity for pursuing their studies during the present month at the University

Laboratories of Oxford and Cambridge, where special courses for County Council students have been arranged.

During the summer, open-air "Lessons in Gardening" are being given also at other centres, and the attendance of thirty or forty people at each demonstration has exceeded the expectations of the Committee.

In all this rush for technical education it is necessary to carefully avoid pandering too much to the tastes of the practical man. Unless the science is kept well to the front, the teaching degenerates into a mere catalogue of dry operations as dead as the average gardener's manual; this does, indeed, help its student to grow various kinds of produce, but the result is due to the inherent power of Nature to assert herself under adverse circumstances, rather than skill or intelligence on the part of the gardener. It is time we lifted gardening and agriculture out of the blind empirical stage, and this cannot be done by mere repetition of operations, however sound a basis these may have. Great praise is due to the Royal Agricultural Society for its recent efforts to spread scientific teaching in the preparation of a simple and reliable text-book on "Agriculture," by Dr. Fream, under their authority. We hope to follow up this example by a "Structure and Physiology of Plants," for the benefit of the gardener.

The machinery for this huge scheme of Technical Education cannot possibly be perfected or completed in a day. Let us hope, however, that the work already begun will develop into a system which will produce as intelligent and skilful a workman as the best of our Continental competitors, and at the same time be peculiarly English in character.

J. PERCIVAL.

SOME NEW BOOKS.

VOLCANOES: PAST AND PRESENT. By Edward Hull, M.A., LL.D., F.R.S., Examiner in Geology to the University of London. *Contemporary Science Series.* London: Walter Scott, 1892.

THIS volume is published in good company; it belongs to a series which has hitherto well maintained its claim to represent the state of contemporary science. It is not without other credentials,—the author is a Fellow of the Royal Society, a doctor of laws, and was, until lately, the Director of the Geological Survey of Ireland. We shall therefore confidently expect to find in it many things both new and true; indeed, the author expressly promises in his preface "the most recent conclusions regarding the phenomena and origin of volcanic action," illustrated by "examples drawn from the districts where these phenomena have been most carefully observed and recorded under the light of modern geological science." A past Director of Geological Survey, writing with these aims, may be fairly expected to know something of the volcanic rocks of which he makes frequent mention throughout the book, yet there is something in his use of terms which, as we read, begins to excite uneasy suspicions, and leads us at last to consult the Appendix, in which is given "A Brief Account of the Principal Varieties of Volcanic Rocks." Let us examine this "in the light of modern geological science." Whatever arguments can be adduced to prove that granite is essentially a deep-seated rock, are equally applicable in the case of gabbro. Every English geologist might have been presumed to have known this much since the appearance of Professor Judd's epoch-making work on "The Ancient Volcanoes of the Highlands of Scotland," published so far back as 1874; yet on page 261 we read that gabbro *is* a volcanic rock, and a little later that granite *is not* a volcanic rock. We regret that we are not favoured with reasons for this distinction; they would no doubt be interesting in the light of "Contemporary Science." We shall discover more contemporary science in the "Tabular View of the Chief Igneous and Volcanic Rocks" given in the Appendix: the well-known sub-division into Basic, Intermediate and Acidic rocks is adopted, but, altered and unaltered, volcanic and plutonic rocks are all jumbled up together, as though each were a species of the same value. But this is a trivial defect in comparison with graver errors; thus we find the Porphyrites placed in the Basic group, and the Andesites, of which they are the altered equivalents, in the Intermediate group, along with Syenite and Mica-trap, while Trachyte is separated from its plutonic representative, Syenite, and placed with the Acidic rocks in company with Domite and Phonolite. And this is Contemporary Science!

Had the author only consulted Professor Judd's work on "Volcanoes," published in the International Science Series in 1885,

which is, indeed, a work of Contemporary Science, he would have been saved from these and worse errors. Perhaps it is because the author proposes to give only the more important varieties of igneous rocks that he omits all reference to the interesting group of Peridotites; but if so, on what grounds is Domite, a mere variety of Trachyte, given such undue prominence? In the definitions of the different kinds of igneous rocks errors are no less plentiful than gross. Basalt is assigned the "high" specific gravity of 2·4 to 2·8; familiarity with the specific gravity of igneous rocks would have rendered such an error as this impossible, 2·4 is a low specific gravity, and the numbers should run from 2·8 to 3. Of Diorite we read that mica (which species?), epidote, and chlorite are present as "accessories." This abbreviation, which recalls the police news, is hardly fairly employed in the case of the last two minerals, which, using the author's phraseology, should rather be spoken of as "secondaries." Syenite is referred to the "Felstone family," though what this family is there is nothing in the work to show; to the same mysterious family the Mica-traps are assigned, also called Lampophyres, so spelt in each of the three or four places where the word occurs; but we must humbly apologise for noticing a trivial error in spelling; there are much better things in the syntax, to which we refer our readers for real enjoyment. In defining Andesite, the author cannot go far astray, as he here commits a plagiarism (no doubt unconscious) on one of those authorities (Dr. Hatch) whom he "is unable to follow," either in classification or definition. Them we cannot commiserate! Trachyte is defined as consisting of certain minerals, which are enumerated, and again "accessories," a style of writing which reminds us of Sam Weller's "leg of mutton and trimmings." We are grateful, however, for the information that the mica is biotite, since, for the most part, the unfortunate mica is made to do duty for all its varieties, like a maid-of-all-work. Here, however, a delicious passage occurs, of which we would by no means deprive our readers; we need merely quote "The trachytes occupy a position between the pitchstone lavas on the one hand, and the andesites and granophyres on the other." And this is Contemporary Science! In a footnote, Mr. Allport is said to have discovered "Leucite basalt" in the Wolf Rock. In another place the author rightly refers to this rock as Phonolite.

Under the head of granite we learn that the quartz is the paste or ground mass in which the felspar and mica crystals are enclosed, and this after the elaborate descriptions of this rock which have lately appeared in the English language; further, that "its representatives among volcanic ejecta are to be found in the granophyres, quartz-porphyries, felsite, trachyte(!), and rhyolites . . . and to one or more of these the so-called granites of the Mourne Mountains, of Arran Islands, and of Skye are to be referred." And this is Contemporary Science. Truly the author has the names of Judd, Teall, Rosenbusch, and Zirkel on his lips, but their precepts are far enough from him.

The Appendix closes with some plates, which it is hoped may providentially prove of assistance to somebody. If we turn to the body of the work we shall find the same Babel of terms, thus (p. 70) the Isles of Lipari are said to consist "of pumice and obsidian passing into trachyte." Surely if the author is not familiar with augite andesite, which enters into the composition of these islands, he has at least heard of Liparite, and this should have prevented him from speak-

ing of obsidian passing into trachyte: on page 119 the hornblende andesite of the Wolkenburg is styled trachyte, and the same term does duty for the tephrite of Niedermendig: on page 234 granophyre is said to be a variety of andesite! And this is Contemporary Science!

The greater part of the work is a compilation of descriptions of volcanoes and volcanic areas, and is certainly not salted with the salt of modern science. Before commenting on the grave errors that render this part of the work really misleading, let us point out a few of those small blemishes which may serve like a rash to diagnose the disease within. On p. 63 we read "Val del Bove (Valle del Bue)"; little pedanticisms like these may be an adornment when correct —but when they are wrong! Let us have it Valle del Bove (Val di Bué). On page 198 the discovery of the volcanic nature of the rocks of Llyn Padarn is attributed to the Rev. J. F. Blake, though Professor Bonney had preceded him in this matter by some ten years (Bonney, On the Quartz-felsite and Associated Rocks at the Base of the Cambrian Series in N.W. Carnarvonshire, *Quart. Journ. Geol. Soc.*, vol. xxxv., p. 309, 1879). On page 68 we are led to understand that we owe our knowledge of the chemical and microscopical characters of the lavas of Vesuvius to Haughton and Hull. The chemical analyses of the first-named author are of undoubted value: they are cited in standard works, but the microscopical investigations of Hull do not seem to have obtained, owing to some reprehensible oversight, the same recognition. Errors are so congenial to our author that he actually goes out of his way to make them. There was no pressing necessity to explain the sky-effects following the eruption of Krakatoa; the author says as much, yet on this difficult problem he cannot be silent. After reading his explanation on page 214, let the reader turn to the elaborate discussion on sky-glows in the Report of the Royal Society—verily a little knowledge is a dangerous thing! To account for the order of emission of the different kinds of igneous rocks from a volcano the early crude form of Durocher's hypothesis published in 1857 is resuscitated, and no mention is made of Richthofen's results (1868) and the explanations of Teall and others. And this is Contemporary Science! On the hypotheses of volcanic activity ventilated by our author we cannot afford to dwell, they are expressed in such loose and unscientific language that it is difficult to get at their meaning with any approach to precision. To anyone who thinks it worth while to consider them we will utter only one caution, let him first make himself acquainted with the strength of materials under pressure and tension, and he will be proof against many of the fallacies of this part of the work.

Where the author is describing things immediate to the senses, he wields, not unskilfully, a fluent pen, which might not discredit G. A. Sala, but directly phenomena involving exact observation, or depending on inductive reasoning for their explanation, are concerned, his efforts are painful. Let us take, for instance, his account of volcanic eruptions. An unscientific writer might be pardoned for an impressionist description of phenomena, and we do not greatly blame a newspaper writer for colouring his accounts of "burning mountains" with "fires" and "flames," but this is not science, and the work before us claims to describe phenomena "which have been most carefully observed and recorded under the light of modern geological science." Just, therefore, is our indignation when we find our author ignoring the true cause of the volcanic glow, and indulging in all the

paraphernalia of "burning mountains," "flames," "smoke," and "fire," and not only so, but inventing a new error, as where he enhances the brilliancy of his pages with "illuminated gas." No doubt exists about the occurrence of flames during an eruption, but they are so feeble that acute observers like Spallanzani and von Waltershausen were led to deny their existence. But Pilla described them so far back as 1833, and they have repeatedly been seen since, one of the last exact accounts of them being that of Fouqué; but they do not play a prominent part, and certainly do not explain the volcanic glow. For a modern writer to thus recklessly revive ancient and long-exploded errors under the name of "Contemporary Science," is to commit no less than a crime against science. We should not have wasted so much space over this work, which was, perhaps, not intended to be taken seriously, but for our fear that it will, in many partially instructed minds, sow the seeds of error, and we abstain from pointing out its lost opportunities; those sins of omission which are as grave and numerous as those of commission. We have now done with our author, whose ignorance of Contemporary Science is of that most fatal kind, the invincible ignorance which is ignorant even of its own existence.

THE ETIOLOGY AND PATHOLOGY OF GROUSE DISEASE, FOWL ENTERITIS, ETC. By E. Klein, M.D., F.R.S. Pp. xii., 130. 53 illustrations. London: Macmillan & Co., 1892. Price 7s.

IT would be hard to point to any investigation of disease and its cause showing greater acuteness of apprehension, honesty of method and resourcefulness in direction than this research of Dr. Klein's. The elusive *causa causans* of grouse-disease has at last been captured. The only certainty about the matter in the minds of those who have tried to grasp the cause of this disease has hitherto been the insufficiency of the late Dr. Cobbold's theory of parasitism by a nematoid worm. Grouse that die of this disease beyond doubt very frequently exhibit the presence of this and other parasites, but these parasites also occur in other birds not subject to it; they are not invariably present, and, above all, the dead birds frequently appear plump and without sign of emaciation. Suggestive contributions to the solution of the mystery were made by Drs. Colquhoun, Macdonald, Farquharson, and Mr. Andrew Wilson, but it was left to Dr. Klein to capture the bacillus, cultivate it, and successfully inoculate mice and birds with the virus. The great difficulties attending the inoculation of grouse have prevented the absolute demonstration of the matter, since inoculation in an infected area would prove nothing, and their inoculation on a moor free from the disease would be somewhat expensive, to say the least, while the difficulties with regard to tame grouse are hard to surmount. It will hardly be objected, however, to this research that there is any lack of corroboration, since the inoculation experiments on other animals adequately answer such objections. There are, moreover, very interesting and hopeful observations with regard to an attenuated virus.

The accounts of a pathogenic bacillus resembling that of grouse disease, of fowl enteritis, and of "cramps" in young pheasants are all masterly contributions to the rapidly-growing knowledge of disease of this class. There is nothing so impressive in the book as the deadliness of aim shown by its author throughout—it works up to a positively artistic effect when the reader comes to the final investi-

gation. One can scarcely pay higher tribute to Dr. Klein's genius as an investigator than by describing his book as a very readable one—in spite of the unfortunate fact that it is almost as badly written as any scientific book in the English language. That invites many odious comparisons, but it is surely time to make protest when so admirable a work in other respects appears seamed with disfigurements of this kind. Finally, the illustrations and "get up" of the book deserve a very hearty word of praise.

THE PHYSIOLOGY OF THE INVERTEBRATA. By A. B. Griffiths, Ph.D. Pp. xvi. and 477. London: L. Reeve & Co., 1892.

IT is not often that one receives such a thoroughly disappointing book as Dr. Griffith's Physiology of the Invertebrates, for while a reliable compilation upon this subject has long been a desideratum, Dr. Griffith's work is so scrappy and untrustworthy that it cannot be recommended to the students to whom it ought to be of most use. The book consists of thirteen chapters, which deal in succession with introductory principles and classification, the chemistry of protoplasm, digestion, absorption, the blood, circulation, respiration, secretion and excretion, nervous systems, special sense, locomotion, and reproduction. The chemical side of the subject is the best treated; the illustrations of the apparatus employed, and the tabular summaries of the results of recent work, such as those on the digestive processes, are certainly useful. The author, however, here assumes far too much knowledge in his reader, *e.g.*, on p. 11 he talks about the specific rotary power of albumen without the slightest explanation for the non-physical student. In other departments the author is less successful; the value of his classification may be judged from his retention of the Eurypterids and Trilobites among the Crustacea, of *Actinophrys* in the Radiolaria, of the Edrioasterida and Rugosa as separate classes, his inclusion of the Hirudinea in the Annelida (p. 256), and his obsolete classification of the Mollusca. This weakness in knowledge of Systematic Zoology is only here of consequence as it leads the author into a series of serious physiological blunders: he makes statements about whole classes which are only true about some members or divisions of those classes. Thus, on p. 34 he puts the possession of an anus as among the characters of the Asteroidea, and of a jaw apparatus among those of Echinoidea; on p. 414 he says that the latter have five genital glands. In many important points, moreover, the author's views are very antiquated and striking recent researches have been omitted. Thus, he states on p. 368 that the Polyplacophora have no eyes, in neglect of Moseley's memoir; his remarks on the eyes of the Echinodermata are rendered valueless by the absence of reference to Sarrasin's discovery of a series of eyes in *Diadema*; the section on the nephridia of the worms is also sadly incomplete, and the few references to papers given suggest a scanty acquaintance with the literature of the subject. In the chapter on respiration, the orthoptera are the only insects to which is referred cloacal respiration; the tissue respiration of the Asteroids is well described, but this is counterbalanced by the omission of reference to the dermal branchiæ. The statement (p. 243) as to the renal function of the pyloric pouches of the Echinoids is somewhat premature, especially in face of the absence of reference to the renal nature of the so-called "heart." There is all through the book a considerable amount of dogmatism; thus, in

dealing with the nervous system he claims unhesitatingly "the origin of the vertebrate brain and spinal cord as the outcome of the nervous system of the Arthropoda"; the space devoted to these remarks might well have been occupied by more precise descriptions of the Arthropod nervous systems, and some attempt to explain to the student the difference between the archicerebrum and syncerebrum, neither of which terms are as much as mentioned. The omission of any reference to the nervous nature of the radial cords in the Crinoids is also unfortunate; the eight pages devoted to the nervous system of the Echinodermata well illustrate the casual nature of the author's method, for with the exception of a few remarks about the eyes of the Starfish, the whole section is restricted to the one type *Echinus*, while the other classes are quite neglected. Extracts of great and unnecessary length are made throughout the book, but it seems difficult to see on what principle the author has made his selections. We should imagine he had bought up a job lot of Mr. Romanes' pamphlets and incorporated most of them. Probably the most serious omission is that there is absolutely no reference to the phagocytes, and this alone would be quite sufficient to condemn any book on Invertebrate Physiology.

LIFE IN MOTION. By J. G. McKendrick, M.D., &c. London: A. and C. Black, 1892.

PROFESSOR MCKENDRICK'S book (of 200 pages) is written in a pleasant and popular style, such as we might fairly expect from the author. His lectures at the Royal Institution, substantially reproduced in this little book—as, indeed, the occasionally interspersed "my young friends" reminds us—were probably even more interesting than the report of them before us. Some of the effect of the subject is lost, for apparatus is much less convincing in pictures than when seen in action. With the exception, however, of the phrase quoted above, and repeated more than once, the book is not written in lecture form, and is all the better for that. "The delivery of the lectures," says Dr. McKendrick, "was a pleasure to myself"; and he expresses the hope that, when in print, they will interest not only the young, but "even those who may feel that they are no longer young." We ourselves are in the sere leaf and belong to the latter category, and we have undoubtedly been most interested in reading the work. Its object is to give an idea of the principal facts in muscle physiology; the title, therefore, is a little misleading, as there is no account of ciliary movements. Though professedly written for young persons of all ages, we notice an occasional lapse into unexplained technicalities; thus, on page 163, the author speaks of an instrument which has a resistance of 86,000 ohms, an expression which can hardly have been understanded of the people. Besides the illustrations of apparatus there are numerous cuts, illustrating other points; diagrams, for example, are given of the musculature of the human arm and the frog's leg; *apropos* of the arm muscles, Dr. McKendrick intimates that young ladies, although they possess a biceps, are not so much interested in its welfare; in these days of lawn tennis, of lady champions, and of Girton College, we are disposed to think that the biceps is greatly in favour with the other sex. On page 79 is a reproduction of a tracing of the muscle curves, obtained during the writing of a sentence, which illustrate the innumerable contractions necessary to make up what appears to be a single effort on the part of the muscles; the curiously shaky result which is thus produced is indeed

suggestive of a New Year's entertainment, but not at the Royal Institution. Chapter V. contains some valuable applications of physiology to practical life; it is reassuring to those of us who are compelled to lead a more or less sedentary existence to learn that the best form of exercise is walking. But if we are to select that form of exercise which is most in accord with our physical organisation, we ought not to forget that the arms should be used as well as the legs; Dr. McKendrick omits to mention that according to "them perfane Darwinists" climbing would probably be an excellent pursuit; the safest and best imitation of this is afforded by the Gymnasium.

THE MICROSCOPIC DETERMINATION OF ROCK-FORMING MINERALS. Tabletzui dlya Mikroskopicheskagho opredyeleniya porodoobpazuyushchikh Mineralov. By F. Loewinson-Lessing. 4to. Pp. xii. and 16, and folding tables. St. Petersburg, 1891.

As an indication of the great strides now being made in Russian geology, we are glad to be able to call attention to this admirably-compiled set of tables of the rock-forming minerals, which, but for the fact that they are written in Russian, would probably prove of greater service to general students than either of those previously issued. Dr. Loewinson-Lessing points out in his preface that he has been impelled to write this work by the fact that so many Russian students are unable freely to use those in French and German. He has, however, arranged the work on a plan that seems a great advance on those previously attempted. The tables of Rosenbusch Michel-Lévy, and Lacroix, and the earlier ones of Doelter and Hussak, give the name of a mineral first, and after that its characters; thus, if we know the mineral, these give us an admirable method of verifying the determination. But if a student is quite at sea as to the name of some mineral new to him, these tables give him but little assistance. In Dr. Loewinson-Lessing's tables, however, the method is reversed; if you know the characters of the mineral you can determine its name. In addition to a large synoptical table, he gives five tables, each using one set of characters. The first table is based on examination in polarised light; the minerals are grouped as opaque, semi-transparent, and transparent; the last is divided into colourless and coloured, and, by aid of the nature of the extinctions, the pleochroism, notes on the general aspect, &c., the species may readily be traced. The remaining tables are devoted to the external morphological characters, the discrimination of the crystalline systems and their application, and the more technical optical characters. As these tables are intended mainly for students and general working geologists, only those minerals are included which are of practical importance. A little over 80 are included, and the long lists of little-known minerals that fill up the tables of Rosenbusch and Michel-Lévy, are wisely excluded. Dr. Loewinson-Lessing's experience, both as a teacher and a skilled petrologist, is sufficient guarantee of the wisdom of his plan and the accuracy of its execution.

ENGLISH BOTANY. Supplement to the Third Edition. Part II. (Orders xxiii. to xxvi.). Compiled and illustrated by N. E. Brown. 4to. Pp. 59–160, with one coloured plate. London: G. Bell & Sons.

IN NATURAL SCIENCE for June we noticed Part I. of the Supplement to the Third Edition of English Botany. Part II. is now to hand,

and is, like its predecessor, the work of Mr. N. E. Brown, of the Kew Herbarium. Of the 104 pages, a few lines are given to a note on *Acer campestre*, ten pages to the Leguminosæ or, as they are styled, Leguminiferæ, while the remainder is taken up with the Rosaceæ, and almost entirely with the two genera *Rubus* and *Rosa*. Mr. Brown admits that he has never studied these two difficult genera, and can, therefore, express no opinion. Professor Babington has revised the proofs of *Rubus*, besides supplying notes for some of the species, while in the Roses Mr. Baker's grouping of the varieties has been followed. A good deal has been done in these genera since the publication of the third edition, and we are inclined to think the present account will form one of the most valuable parts of the Supplement, even though the author does not venture to express his own opinion and it be but a compilation. Mr. Brown is a painstaking compiler, and has had work of the very best from which to draw, and some British botanists may set a higher value on such compilation than on more original notes, for British botany must be learnt in the field, and the many varieties and critical forms studied as they grow. Conclusions drawn from herbarium specimens are not always borne out by the living plant, and although the facts relating to plants and their synonymy have been assiduously collected for the Supplement, yet we seem to miss the matured experience and confident criticism of the practised field-worker.

The paper and printing are as excellent as those of the previous part, while the solitary plate (*Potentilla norvegica*) is as poor as its predecessors. It looks wiry, and is evidently not the work of a competent artist.

GRASSES. By C. H. Johns, M.A. Fcap. 8vo. Pp. 96. London: S. P. C. K. Price 1s. 6d.

THIS little book is styled "An Appendix to the late Rev. C. H. Johns' Flowers of the Field"; referring, of course, to the well-known work by the Rev. C. A. Johns, also published by the S. P. C. K. The title is misleading, as a third of the work is occupied by the Sedges or Cyperaceæ, while the decoration of the cover with the common Bulrush (*Typha latifolia*), a member of an Order not very closely related to either Grasses or Sedges, is hardly commendable.

A handy popular flora devoted to these glumaceous monocotyledons has long been a desideratum, but we fear the arrangement and descriptions in the present work are not sufficiently popular to supply the want. In "get up" it is distinctly inferior to "Flowers of the Field," the letterpress is not so clear, and the illustrations, which form so essential a part in such a flora, are very poor. It is a pity we cannot have a new edition of the whole work brought up to date, and including not only Sedges and Grasses, but also the Ferns and allies. The thirty pages occupied by an account of the Linnæan arrangement might be left out, and some of the space devoted to a fuller explanation of terms now rather crudely disposed of in 14 pages.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE German Anthropological Society holds its annual meeting at Ulm on the 31st July.

M. A. F. BATALIN has been nominated Director of the Botanical Garden of St. Petersburg in the room of the late E. Regel.

THE International Congress of Physiologists will take place at Liège, from the 28th to 31st of August.

A NEW Zoological Museum is shortly to be built at St. Petersburg, and the Imperial Academy has sent Dr. Pleske to inspect the principal museums of Europe to report thereon. A paragraph in our last issue announced a similar enquiry on the part of the Japanese Government.

THE Natural History Museum will shortly receive a piece of the skin, with wool attached, of the Mammoth. This specimen forms part of an exchange with the Imperial Academy of St. Petersburg, and comes from an individual discovered in the province of Jakutsk, Siberia.

THE Imperial ukase requiring that all lectures given in Dorpat shall be delivered in the Russian language, instead of in German as hitherto, comes into force next year. This necessitates the resignation of several of the Professors, including, among others, Professors Lemberg and Rosenberg, who hold the chairs of Geology and Comparative Anatomy respectively.

THE National Chrysanthemum Society are expecting an interesting arrival from New Zealand. Mr. John Earland, of Wellington, having raised some chrysanthemums from seed, is anxious to obtain the Society's opinion on the blooms. He has accordingly made arrangements to have them frozen in blocks of ice at the meat-freezing works and forwarded to London in the meat steamer. As soon as the ship arrives, a meeting of the General and Floral Committee is to be called to examine and report upon the flowers.

THE Professorship of Geology and Mineralogy at Breslau, left vacant by the death of the late Professor F. A. Römer, is to be subdivided: The Professor Ordinarius will occupy the chair of Petrology, and the Professor Extraordinarius will have charge of the department of Palæontology. It is considered probable that Dr. Fritz Frech, of Halle, one of the leaders of the younger school of German geologists, will receive the latter. Dr. Frech is well known as the Monographer of the Triassic Corals, and as a writer on past zoological distribution.

MR. F. A. BATHER, of the Geological Department of the British Museum, has been awarded the Rolleston Scholarship for his work on Fossil Crinoids and the Phylogeny of the Cephalopoda.

IN the recently-issued list of Doctors of Science of the University of London are included the names of Mr. Bernard Dyer, the well-known agricultural chemist, and Professor T. Jeffrey Parker, of Otago.

MR. J. BRETLAND FARMER, M.A., Fellow of Magdalen College, Oxford, and Demonstrator of Botany in the University, has been appointed to succeed Dr. Scott as Lecturer on Botany at the Royal College of Science, South Kensington, Dr. Scott having undertaken the charge of the Jodrell Laboratory at the Royal Gardens, Kew.

MR. F. A. BATHER contributes a paper to the last Report of the Museums Association on the Fossil Crinoids in the British Museum, in which he describes his "attempt to put into practice modern ideas of museum arrangement." He accepts as his rule the American maxim that a museum is a collection of labels illustrated by specimens. The paper mainly consists of a reprint of the thoughtfully prepared labels he has attached to the collection, and will serve as an admirable model for educational museums. He also makes the sensible suggestion that museums should exhibit more chairs, and a few light "dumb waiters on wheels" for the use of visitors. He thinks, however, the use of these as go-carts by children should be discouraged.

WE have received from Dr. A. Jentzsch a copy of his "Führer durch die Geologischen Sammlungen des Provinzialmuseums der Physikalisch-Oekonomischen Gesellschaft zu Königsberg." This is a pamphlet of 104 pages, illustrated by 75 process blocks, and two stratigraphical tables. The guide serves as an admirable summary of the general geology and palæontology of Eastern Prussia. The most important collections in the Museum are the amber plants, and the inter-glacial mollusca and plants. In the preface short reference is made to the history of the Museum and the principal collections it contains. We wish other German museums would follow Dr. Jentzsch's example.

THE third part of the second volume of the *Journal of the Marine Biological Association* contains several papers of much practical value. The most important is a report by Mr. J. T. Cunningham, "On the rate of growth of some sea fishes, and the age and size at which they begin to breed." The paper is a somewhat long one, and is crowded with statistics respecting the cod, flounder, dab, sole, herring, sprat, pilchard, anchovy, &c. The most interesting addition to the fauna of the Plymouth district is a Siphonophore, which was comparatively abundant last autumn, and even worked its way into the Sound; it is described by Mr. Cunningham as a new species, *Muggiæa atlantica*, and has affinities with a species living round the Canary Isles. Mr. Hughes is still continuing his experiments on artificial bait, but, so far, with few practical results.

WHILE the National Laboratory at Plymouth is naturally devoting most of its attention towards problems having an economic bearing, the Liverpool Marine Biological Association is continuing its splendid work on the fauna and flora of the Irish Sea. As the Association's old station at Puffin Island was rather inconvenient of access, and as five years' steady work has nearly exhausted the fauna of the limited shores of that island, headquarters have now been removed to Port Erin, at

the south end of the Isle of Man. A new volume of the Association's reports upon the Fauna and Flora of the area within the Association's sphere of operations is announced.

THE celebrations in connection with the Tercentenary of Dublin University passed off successfully. Each day, from July 5th to 8th, was occupied by a succession of academical gatherings. The number of delegates and guests was not so large as had been expected; the unfortunate coincidence of the festivities and the General Election prevented the presence of many invited from England, and the representatives of natural science, in particular, were less numerous than had been hoped. The proceedings began on the morning of the 5th with a procession in academical robes to St. Patrick's Cathedral, where a commemoration service was held; in the afternoon the Provost, Dr. Salmon, gave a garden party, and in the evening, after the performance of the Tercentenary Ode, the Lord Mayor gave a reception and ball. On the 6th, after the conferring of honorary degrees, there was a garden party given by the Lord Lieutenant, and, in the evening, a grand banquet by the University at Leinster Hall. On the 7th, the delegates presented addresses of congratulation, on behalf of their universities, to the University of Dublin; another garden party by Lord Wolseley oocupied the afternoon, while the evening was devoted to a performance of the "Rivals" by University students. On the 8th, some of the delegates addressed the students, a party visited Lord Rosse's castle and observatory, near Parsonstown, and in the evening the festivities concluded with the University Ball.

ENTOMOLOGISTS will be interested in an exhibition of tropical Lepidoptera by Mr. Wm. Watkins, of Croydon, now being given at 21 Piccadilly. The collection includes many fine specimens of African rarities, such as a pair of *Papilio antimachus* from the Gaboon, and also several of the Madagascan *Urania rhipheus*. Many striking instances of protective mimicry are shown in the collection, which most entomologists will find worthy a visit.

OBITUARY.

KARL HERMANN KONRAD BURMEISTER.

BORN 1807—DIED MAY 1, 1892.

BURMEISTER was born at Stralsund, and studied medicine at Greifswald and Halle, obtaining his M.D. at the latter place in 1829. In 1837 he became Professor of Zoology in the University of Halle, but resigned his chair in 1850, having taken an active share in the political struggles of the time. He went to Brazil on a scientific journey, and returned to Germany, where he resumed his professorship at Halle. Again returning to South America in 1856, he travelled about the Argentine provinces, and made a journey across the Andes by a new route. Once more visiting his native country in 1860, he finally resigned his chair at Halle in 1861, and became Director of the Museum of Natural History in Buenos Ayres, which he had established and furthered. In 1870 he was appointed Curator of the University of Cordova. Burmeister was chiefly known from his long series of papers on vertebrata, published in the *Anales del Museo Publico de Buenos Aires;* his "Systematische Uebersicht der Thiere Brasiliens" (1856); and his "Description Phisique de la République Argentina" (1876). He died on the 1st May, and received the honour of a State funeral, at which the President attended in person.

MEMORIAL TO PROFESSOR DUNCAN.

A MEMORIAL brass is being prepared in memory of the five members of the staff of Cooper's Hill College, who died last year. Of these the best known was Professor Peter Martin Duncan, and geologists will therefore feel a special interest in this tribute to the memory of one of the most philosophic of palæontologists of our time.

OBSERVATIONS AND CORRESPONDENCE.

Life at High Altitudes and at Great Depths.

[The following remarks have been suggested by Mr. Whymper's conclusions recently noticed in our review of his work (pp. 219-220), but Mr. Whymper has of course considered the question of the experiences of æronauts and dismissed them on account of the suddenness of the alterations of pressure.—Ed.]

Experienced hunters upon the Himalaya pursue game, without feeling any appreciable inconvenience, at altitudes exceeding 20,000 feet above the surface of the sea, and balloonists have penetrated up to 37,000 feet, or fully 7 miles above the sea's level.

Mr. Glaisher in one of his balloon ascents remained in apparent health up to 26,400 feet, or 5 miles, but, on arriving at 29,000 feet, 51 minutes after leaving land, he lost all muscular power, soon followed by insensibility and fainting, possibly due to the glut or engorgement of blood in the lungs from increasing insufficiency of oxygen, intensified by diminished skin-action in a dry temperature of 37° Fahrenheit *below* the freezing point of water.

Hitherto high balloon ascents have been performed with such speed that either 1,000 feet upwards or 4,000 feet downwards have been readily accomplished in one minute, while in mountain travelling an elevation of 1,500 feet is seldom completed within the hour.

Experiments indicate that by gradual ascents, long training, frequent acclimatisation, and cautious diet, high altitudes above 30,000 feet (now considered by many fatal to man) are, with precaution and practice, securely attainable.

The air-hunger experienced by man at high altitudes occurs also in fish when suddenly removed from a very low depth to the surface. Thus, from the experiments of the "Challenger," it appears that fish hooked at great depths often reach the surface in fragments, owing to the disintegration of the tissues, caused by the enormous expansion of the gases. In any case, the fish are dead and rigid, or the stomach evaginated through the mouth; considerable protruberance of the eyeballs may also occur. It is very interesting to observe that when nearing the surface the fish, owing to the expansion of its gases, is literally shot through the water, and even projected above the surface faster than the fisherman can draw in his line.

J. Lawrence Hamilton, M.R.C.S.

30 Sussex Square, Brighton,
1st July, 1892.

Sir,—I was struck by a remark in Professor Lloyd Morgan's masterly critique of the "Grammar of Science" in your last number.

He says that "considering how enormously complex are the influences which have conspired to bring about man as the crowning

product of evolution on this earth, we should judge it to be indefinitely improbable that human beings, as such, have been evolved on any one of those myriad planets" (p. 303).

No one can speak with greater authority on those "enormously complex influences" of evolution than Professor Morgan, but at the same time no one ought, more than he, to recognise that these complex influences have the value of fixed laws.

He well knows that celestial matter seems to respond to the same laws that govern terrestrial; that there are many celestial bodies of a similar constitution to our sun, and surely among their satellites there must be some of a similar kind with our earth.

Though we know nothing of life outside our planet, yet what we know of it here is seen to exist under fixed laws, and surely, granting life outside the earth, we would have it developing along those broad principles we know a little of. That is, in an earthly type of planet, accompanying a solar type of sun, we may expect to find a biological development of, at least, a recognisable similarity to our own. What marvels me most is, that Professor Pearson (the apostle of scientific Nihilism) should have written the passage which Professor Morgan attacks.—I am, yours obediently, J. BELL FINDLAY.

Lewisham, S.E., *June* 26, 1892.

DEAR SIR,—I send you a copy of a notice which is posted in the bridle-path which runs along the foot of White Hill, near Caterham. What will the poor London botanist do if this becomes the general rule with South London landlords?

"£2 Reward to anyone giving information leading to conviction of persons stealing Berberis or other things, damaging shrubs, trees, fences, &c., *gathering roots*, *flowers*, or otherwise trespassing on the Estate. *Dogs will be used* day or night, if necessary, to help the capture of such offenders."

Fancy the innocent botanist pursued by an infuriated landlord and a pack of dogs. You will, I am sure, kindly warn all Rambling Clubs to avoid picking buttercups and daisies in the neighbourhood of Quarry Hangers, White Hill.—Yours truly, R. PAULSON.

SOME HABITS OF BIRDS AT THE ZOOLOGICAL GARDENS.

IT is unnecessary to point out at length the expediency of paying a close attention to habits and movement in the study of a group like the birds. The minor "tricks," as they may be called, are often suggestive of affinities in a group where these are especially hard to diagnose; and again actions are often witnessed which afford valuable indications of the way in which complete and important changes of habit have been effected. The London Zoological Gardens offer especially good opportunities to anyone desirous of acquainting himself with the traits and manners of many groups of birds; and in many cases also departures from these may be noticed, which for the reason above given are equally interesting. Gulls, for instance, have been noticed, when at large, to not unfrequently perch on trees; and in the large netted inclosure known as the Night Heron's Pond this may be often seen, the Common and Black-Headed gulls there confined appearing to find the top branches of

the dead trees a convenient perch. The single rather deep pond which this enclosure contained has been rightly done away with, and three smaller and shallower ones substituted; in which old pond I have more than once been interested by observing the Night Herons swimming—usually for only a few strokes, it is true, when they got out of their depth in bathing or reaching after floating food. But on one occasion one bird, driven into the water from an island by another, swam quite easily and composedly to the shore. Although the Common Heron is well known to be capable—as apparently are most birds of wading habits—of natatorial feats, I am not aware that similar performances have been before observed in the case of *Nycticorax griseus*, which sits as lightly on the water as a gull.

These occurrences are interesting, as showing how a shore bird might gradually and naturally, through seeking food on steep banks, become a swimmer; and others, noticed in these gardens and elsewhere, tend to show that the surface-feeding Anatine ducks could easily develop a diving habit. In the Northern Pond, last year, I was fortunate enough to see a male Mandarin Duck (*Aix galericulata*) dive repeatedly and energetically for food, and Mr. C. Bartlett informed me that the Summer Duck (*A. sponsa*) will likewise do so. The young of the Chilian Pintail (*Dafila spinicauda*), yet in down, repeated the performance elsewhere in the gardens at another time. This accomplishment may prove to be more common among the *Anatinæ* than is generally supposed; on four occasions I have detected it in the half-domesticated Mallards in the London parks, the birds being in one case ducklings in down. Swainson states that the Gadwall (*Anas strepera*) dives as easily and frequently as the sea-ducks; but I have not yet witnessed the action on the part of this species, though examples are at present in the Society's collection. The distinct little group of Tree-Ducks appear also to have the plunging habit, which I have noticed in the case of three species (*Dendrocygna major*, *D. vagans*, and *D. viduata*) before the deep pond in the Night Herons' quarters, where they are placed, had been altered. Of course, the surface-feeding ducks dive when in play, but the action in that case is very different, and easily distinguishable from the purposeful plunge of a feeding bird.

That the Tree-Ducks should dive is not to be wondered at, as their general build and the large size of their feet, in spite of their long eggs, distinctly recall the Fuliguline subfamily.

While on the subject of the ducks, it may be mentioned that the Common Foxes in the Zoological Gardens readily ate a specimen of the Common Sheldrake. As this bird is reputed to nest unharmed in an occupied fox-earth, and to be more than indifferent human food, it seemed possible that its very conspicuous appearance, in both sexes, and even in the first plumage, might possibly be of warning significance. It would be, however, very desirable to repeat the trial with a perfect specimen, as the bird given to the foxes had been plucked and disembowelled, circumstances having prevented me from carrying out my original intention of making myself the *corpus vile* of the experiment. I have no doubt, however, that I should have been able to discuss Sheldrake with toleration, as I could not discover any undue unpleasantness, or even fishiness, in the flavour of the Red-breasted Merganser or the Great Crested Grebe, to say nothing of the Scaup and Golden-eye, which appear not to be considered very good eating.

Two Red-billed Hornbills (*Toccus erythrorhynchus*) being at present

in the Parrot House, there is a good opportunity of comparing the smaller species of this interesting family with the larger forms to be found in the Eastern Aviary. Their greater activity is especially striking, and they do not seem to object to coming to the ground, where they hop about quite nimbly, in a manner very unlike the clumsy jumps of the Elate Hornbill (*Ceratogymna elata*) on the other side of the gardens. They are also fond of meal-worms, and it is probable that these small species generally are more insectivorous and less exclusively arboreal in their habits than the larger, a fact not without interest, with regard to the relationships of this family to the Hoopoes.

Why some birds should hop and others walk when on the ground it is rather difficult to say; and authors are not always very correct in describing the gait of birds. The more arboreal forms seem, as a rule, to be most given to this mode of progression; yet parrots walk, and the common sparrow, so much upon the ground, always hops. I particularly noticed, when the Kea (*Nestor notabilis*) was first brought to the gardens, that it distinctly walked on the floor of its cage, for, from accounts given, this genus would appear to progress by hops assisted by the wings, as may be seen in many birds when they wish to advance rapidly. But in the limits of a cage one could hardly expect to see this action; since it is not at all unusual to see the same birds advance sometimes by running and sometimes by jumps, when they have space—the Crows and Touracous for example; and the alternate hop and run of the Thrushes is familiar to everyone.

That very peculiar form, *Struthidea*, at present in the Parrot House, is a walker; and also may be seen to crack canary seed like a finch, a curious action in a bird of its decidedly corvine appearance.

A comparatively recent addition to the collection in this house is a specimen of the Sulphury Tyrant (*Pitangus sulphuratus*) which seems to display the adaptability of appetite characteristic of birds of this form, a portion of mouse and some butterflies (*Pontia*) appearing equally acceptable.

FRANK FINN.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 67–69 *Chancery Lane, London, W.C.*